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THE PSYCHOLOGICAL REVIEW

THE OPERATIONAL DEFINITION OF CONCEPTS [1]

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Periods of rapid progress in science invite attention to the meaning assigned to its basic concepts. This is notably true at times when, like the present, more accurate methods of investigation extend knowledge into fields hitherto unexplored. In physics, the discovery of new regions for investigation has raised anew the problem of method as well as that of the nature of subject-matter itself; indeed, at the hand of Bridgman (4) many physical concepts already have undergone certain alterations in meaning. Not to be outdone, a number of prominent psychologists have been stirred to demand a cleansing of the Augean Stables in their own domain. In both physics and psychology we are invited to adopt the 'operational' view of the meaning of concepts, *in lieu* of the traditional definition of them in terms of properties considered as independent of the modes of investigation whereby they are discovered. In physical theory, Bridgman (4) has formulated operational definitions of space, time, length, and other concepts. In psychology, Boring (3) has devised an operational definition of temporal perception; McGeoch (7), of learning; Carr (5), of learning, forgetting, and certain related terms; McGregor (8), of identity and numerical equality; Stevens (10, 11), of equality, definition, meaning, existence, experience,

[1] We take pleasure in acknowledging our indebtedness to Waters and Pennington (13), especially as regards references to the psychological literature bearing on our topic. While we are in general agreement with the criticisms of operationism offered by these writers, we have attempted to describe and criticize the movement from a different point of view.

sensation, and attribute; Seashore and Katz (9), of mental mechanisms; and Tolman (12), of drives.

We offer no criticism of the definitions given of these concepts. Instead, we shall be concerned with the nature and implications of the method of operationism as exemplified in these definitions, and, in the light of this analysis, to evaluate its adequacy as method. Since all definitions of operationism turn upon the meaning and rôle assigned to the word 'operation,' we turn first of all to the various definitions of operationism formulated by the above-named writers.

"In general," writes Bridgman (4), "we mean by any concept nothing more than a set of operations; *the concept is synonymous with the corresponding set of operations.*" As stated by Benjamin (2), "concepts must be defined in terms of the processes by which the objects in question are found." As stated by McGeoch (7), "meanings can be more adequately defined in terms of the operation of measurement than in terms of phenomenal properties." McGregor (8) defines operationism as "the belief that an entity is adequately defined only in terms of the specific operations involved in its observation." For Stevens (10, 11), "operationism consists simply in referring any concept for its definition to the concrete operations by which knowledge of the thing in question is had —or is arrived at."[2] Seashore and Katz (9) assign a wider meaning to the term. "Operational definitions," they affirm, "may be of two main kinds, both of which describe what the person must do to produce or recognize the phenomenon." Thus we may describe the color red "by telling what stimuli should be used to produce it. The definer then 'points' to the result and says 'that is the color red.' This is definition by demonstration. The second type of definition is used in identifying certain phenomena by the occurrence of certain distinguishing characteristics or results of the phenomena. Thus the term 'intelligent' applies to behavior which is *appropriate* to the situation and also efficient in the sense of saving time and energy. The 'operation' of defining then

[2] Stevens (11) goes so far as to propose an operational definition of *operation:* "An operation is the performance which we execute in order to make known a concept."

consists of telling the person *what to do* to produce the phenomenon or what signs to look for in recognizing or distinguishing it from similar cases."

From these definitions it is clear that the meaning of a concept is identified with, *i.e.*, the concept refers alone to, a set of operations. Yet it is also acknowledged that operations (meanings) are determined by the character of the things and events that evoke them. Two questions of basic importance at once arise: (1) precisely *what* does the term operation embrace, and (2) what is the relation between the operation on the one hand and its objective referent on the other? We shall attempt to show that, relative to the first question, operationism as thus far described is too elastic in denotation to warrant any claim of novelty for it, and that, as respects the second question, it is too indefinite in meaning to permit any definite reply thereto. To implement these conclusions, we invite attention to the meaning of the term operation as exemplified in the following statements.

In its generic sense, the term operation denotes, in the words of Lewis (6), 'something done, performed'; it is a response by a person to something. While not all responses are operations, *i.e.*, are cognitive and scientifically significant, an enormous variety of human responses is embraced by the term operation. Benjamin (1) speaks of conscious and unconscious operations, inventions and discoveries. Indeed, he adds, "we may distinguish two general types of operation: (1) an operation which is performed upon an object to produce a symbol; and (2) operations performed upon symbols to produce other symbols." These two types of operation correspond, respectively, to the extensional and intensional modes of definition of traditional logic. Moreover, he declares, "an operation is an act by which awareness passes from one content to another. Thus an operation is a change in the locus of awareness. It might be defined as a shift in attention. . . ." Elsewhere he includes within the denotation of the term such 'common operations as generalizing, classifying, ordering, inferring, measuring, pointing, abstracting, constructing, describing, explaining, and negating.' Moreover,

he insists that "the structure of operations is clearly that of relational complexes." And "there will always be revealed in addition to the operation something which is operated on and something which . . . is called into awareness by the operation."

That the term operation is a coat of many colors is also amply illustrated in the writings of those psychologists who have formulated operational definitions of psychological concepts. Thus Boring (3), in describing the nature of temporal perception, declares "there is no meaning to the assertion of the existence of a temporal perception, except that certain physiological events exist prior to the report and that their differentiation is such as the report can adequately indicate. A perceived duration or temporal pattern is a psychological entity that is inferred from and defined by certain operations of introspective report. . . . This makes the perception, not a private immediate experience, but a psychological *construct* which is just as public as any other convincing inference from data." Hence "the term *immediate* as applied to temporal experience turns out to be meaningless."

Carr's plea (5) for the adoption of an *attitude of relativity* in psychology, particularly in respect to the relative character of the meaning of concepts, also makes use of all the processes which Benjamin recognized as embracing the term operation. Take, *e.g.*, the controversy over the merits of the whole *versus* the part method of learning. "On the relativistic assumption that the merits of the two methods is relative to experimental conditions," writes Carr, "the experimenter then proceeds to investigate such variables as age, training, memory span, the I.Q. of the subject, amount of material and number of parts into which it is divided, the distribution of practice," etc. The coefficients or graphs obtained will then give the answer to this question. Moreover, any variation in the selection of relevant factors, any alteration in the value ascribed to any particular factor, will condition the results obtained. Yet at most this but denies *absolute* value to any concept; it but implies that there is not one curve of learning, but as many as there are diverse conditions under which the investigation is carried out.

One further illustration of the wide use of the term operation will suffice. Stevens (**10, 11**) has formulated operational definitions of some of the more elementary principles of method common to both physics and psychology. Of these *discrimination* is the most fundamental. It is fundamental to the operation of *denoting*, or *pointing* to, for a thing must first be discriminated before it can be denoted. These two are declared to be involved in almost all operations. The meaning of *equality* consists in "considering as equal all aspects of things which show no discriminable difference," or "whenever discrimination of a difference is attempted and fails." *Meaning* "is wholly a matter of operations, and principally of the operation of denoting." A *definition* "is the sum total of the criteria (operations) by which we determine the applicability of a term to any particular instance." *Experience* "denotes the sum total of the discriminatory reactions performed by human beings, for *to experience* is . . . to react discriminatively." In any other sense the term is dismissed as meaningless.

Enough has been said to indicate the extreme ubiquity of the term operation. If it be extended to denote such diverse acts as discriminating, denoting, identifying, equating, generalizing, inferring, constructing, counting, measuring, and all the multifarious mathematical operations employed in scientific investigation, what possible element of distinctiveness can the word 'operation' connote? Is the term invented merely to celebrate the belated discovery that all of the abovementioned acts are operations? If so, then the baby has long since been poured off with the bath. If these acts are labeled 'operations' merely to remind us that concepts do not emerge from the void passively freighted with definite meaning, or, if the term merely reaffirms the fact that some reaction by a person or organism is essential to the discrimination of meaning, then the sole cause for wonder is that any one should think otherwise. In its denotative sense operationism only succeeds in pouring the same wine into other bottles. To rechristen Lincoln *Humpty Dumpty* in no way alters the character and deeds of the historic Lincoln.

Yet operationism claims to be a *new* method in science; some even acclaim it to be a new positivism, destined to revolutionize all science, psychology included. Here we confront the second and more fundamental problem alluded to above: What is the relation between meanings as operations on the one hand and their objective referents on the other?

Now whether a meaningful situation is dyadic or triadic we need not here decide. It is only necessary to distinguish the symbol or concept from the thing signified or meant. In this, all theories of meaning, operationism included, agree. But precisely *what* is meaning; of what does it consist? It is in the answer to this question that we uncover a possible element of novelty in operationism. For, to this question, all operationists, following Bridgman, reply: The meaning of a concept consists in a *set of operations*. Thus the meaning of the term *length of this room* consists in the acts performed in applying some measuring rod to one of its sides. Thus the term *intelligence* consists in the operations performed in the discrimination and measurement of certain patterns of animal behavior. Where no operation is possible, concludes Bridgman (4), there is no meaning.

While this is what he affirms, can this be what the operationist actually means? Take again the concept *length of this room*. Does its length consist solely in the movements of hands, arms, legs, and body, the motions of the measuring rod, etc.? If so, meaning is a mere gesture toward the void. Rather, may we not say that the length of this room is some value which these operations disclose, some *property* (to re-introduce the forbidden word) of the object measured whose approximate value we *discover* by means of the operation of measurement? Put otherwise, do we succeed in measuring the object or merely our reactions to it? In truth we measure both, because it is by counting and assigning measured value to our movements as defined by the measuring rod that we measure something to which our acts refer and which determines their character. Thus operationism is in fact a method whereby we come to assign meaning to a determinate order of things and events independent of the operation. Yet this only

succeeds in making explicit certain limitations to knowledge inherent in relativity theory.

Nor is the case otherwise when we turn to operational definitions of psychological concepts. Consider the perceptual discrimination of the color red from the color blue. Now there can be no objection to calling this act of discrimination an 'operation.' But surely the colors discriminated are not one with the act of discriminating. Yet may we not, as Seashore and Katz (9) contend, describe the color red by telling what stimuli should be used to produce it? Unfortunately, this is to confuse description with causal explanation. That this is not what they mean is evident when they add: "the definer then 'points' to the *result* and says 'that is the color red.'" Yet this is equally objectionable, for it assumes that denoting is a true form of definition. A triangle is not defined either by drawing or pointing to a certain figure on the blackboard.

We believe the basic confusion in the operational definition of meaning is now laid bare. It fails clearly to discriminate the *pointing* from the thing *pointed to;* commonplace though it be, it confuses the nature of the thing disclosed with the operations whereby the disclosure is made. This conclusion is further evidenced by the following considerations.

At times Bridgman (4) seems to identify the operation with the thing operated on. Thus he declares that in making a statement about things, "we are merely making statements about the character of our descriptive processes." When examined in its context it is seen that this statement only intends to deny the possibility of *absolute* [3] knowledge and thereby to affirm universal relativity. Yet he admits that the specific character of our operations is determined by nature. What, then, is the precise nature of this determination? Grant all knowledge to be relative; the problem still remains: Of *what* do we possess knowledge; to *what* do our operations refer. On this issue, unhappily, our operationists remain silent. A favorite evasion of scientists, including many

[3] In the sense that the specific terms in which knowledge of nature is formulated are uninfluenced by the particular factors which condition investigation.

psychologists, is to dismiss this problem as *merely* epistemological or *merely* philosophical. Give a dog a bad name and hang him! But until the nature of this relation is specified, no theory of meaning, least of all operationism, merits serious consideration, since to be a theory of meaning is to give some account of this relation. Here our operationists leave us wholly in the dark.

Yet the problem of the relation of meaning to the thing meant is no artificial and merely verbal dichotomy. To identify the world of nature with meanings as operationally defined is of no aid, since on this assumption all meanings would be initiated wholly internally, and would deploy themselves *in vacuo*. Grant that certain of the meanings attributed to things do consist in the responses we make to them. A knife *means* something to cut with; an apple *means* something to eat, make into pies, press into cider. But to restrict meaning to use would reduce science to *bio-* or *psycho-*centrism or both. While logically this is a possible viewpoint, the history of thought attests its implausibility. We might, *per contra*, take refuge in a Kantian *Ding-an-Sich* or Spencerian *Unknowable*, but this is neither helpful nor consistent with the realistic postulate of operationism.

Thus far the conclusions resulting from our analysis of operationism must be essentially negative in character. Operationism proposes no new methods of investigation; at best it but redefines methods already in general use. To contend that operations are acts performed by some investigator is merely to recognize that scientific knowledge is achieved by active investigation and measurement. To insist that the entire meaning of a concept consists in a set of operations results in confusion; indeed, to identify meanings with acts performed by a human being makes it all the more necessary to specify the nature of the relation between meanings and a world of determinate things and events assumed to hover in the background all the while. But operationism fails explicitly to recognize, much less to offer an intelligible account of, this relation.

Is operationism as method then wholly without merit? It

would, we believe, be a mistake to dismiss it merely as such. However, its real merits lie elsewhere than in those attributed to it by its apologists. We cannot too much or too often reaffirm our faith in experimental methods; indeed, it is because of our failure rigidly to adhere to experimental demonstration that so much of what Plato called *opinion* still masquerades as scientific knowledge. In its insistence upon empirical methods, that knowledge must consist in solutions of problems suggested by, and be verified by data presented objectively to, experience, operationism is both timely and welcome. In its recognition of the *relative* character of all scientific operations and results, so ably argued by Bridgman (4), Carr (5), and Stevens (11), operationism performs a valuable though belated service to science. In its insistence that definitions of concepts should be freed from all extraneous and mystical elements, it is thrice welcome. But as a novel theory of meaning, or as a substitute for methods already in general use, operationism, it would seem, is as yet too confused as respects its meaning and implications to enlist many under its banner.

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PREDICTION OF VICARIOUS TRIAL AND ERROR BY MEANS OF THE SCHEMATIC SOWBUG [1]

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The present report consists of two parts: first, the presentation of some experimental results; and second, the presentation of a theoretical schema for the interpretation of those results.

The Experiment

The experiment was a white-black and a white-gray discrimination experiment but the interest was not simply in the relative ease with which the subjects (rats) learned to differentiate between the correct and incorrect stimulus-objects but also in the relative amounts of 'looking back and forth' or, to use Muenzinger's term, 'vicarious trial and error' (or 'VTE') which they exhibited in the course of acquiring these differentiations.[2] In short, our experiment will be concerned not only with learning scores but also with VTE scores.

Three groups of rats with pigmented eyes (all approximately five months old at the beginning of the experiment) were run in a modified Lashley discrimination set-up. See Fig. 1. For all these groups the correct door was white, but for one the incorrect door was black, for another it was medium gray, and for the third it was a very light gray. The group run on the white-black set-up contained 9 males and 2 females, that on the white-medium gray set-up 8 males and 2 females, and that on the white-very light gray set-up 8 males and 2 females. The males were all run at the beginning and the

[1] Read, in part, before the American Psychological Association, September 7, 1938.

[2] For a discussion and summary of most of what is known to date about vicarious trial and error, much of it obtained in the Colorado laboratory, see Muenzinger's recent splendid report (7). See also Tolman (9; 10, Chap. XIII; 11) and Dennis & Russell (5).

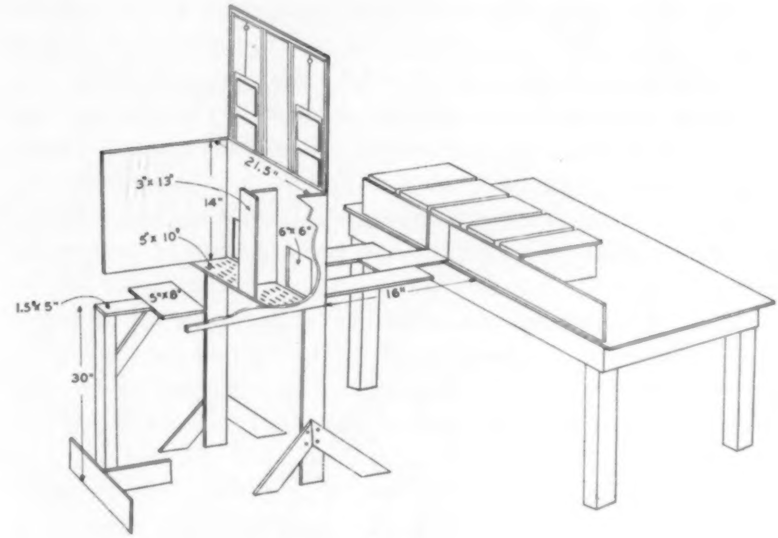
21.5"
3"x 13"
14"
5"x 10"
6"x 6"
5"x8"
1.5"x 5"
16"
30"

Fig. 1

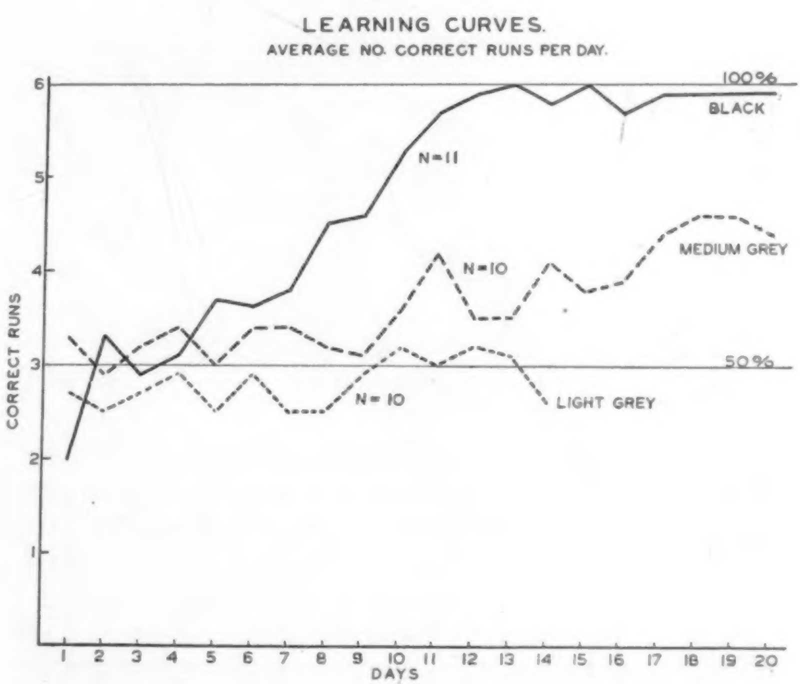
LEARNING CURVES.
AVERAGE NO. CORRECT RUNS PER DAY.
100%
BLACK
N=11
MEDIUM GREY
N=10
50%
N= 10
LIGHT GREY
CORRECT RUNS
DAYS
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
0 1 2 3 4 5 6

Fig. 2

females all run at the end of each day's session. A preliminary training period consisting of seven experimental days in which the animals were taught to jump and to push open the doors preceded the experiment proper. In the preliminary period both doors were white. In the experiment proper every rat was given six trials a day. The positions of the correct door were on alternate days, LRRLLR and RLLRRL for all three groups. The gap during the experiment proper was eight inches wide for the males and six inches wide for the females. The animals were not run on Saturdays or Sundays. They were given a heavy meal in their cages on Saturdays and were not fed at all Sundays. No evidence of any disturbance in the learning curves appeared as a result of this procedure.

The learning curves for the three groups are shown in Fig. 2. The white-black group learned best, the white-medium gray next best, and the white-very light gray not at all.

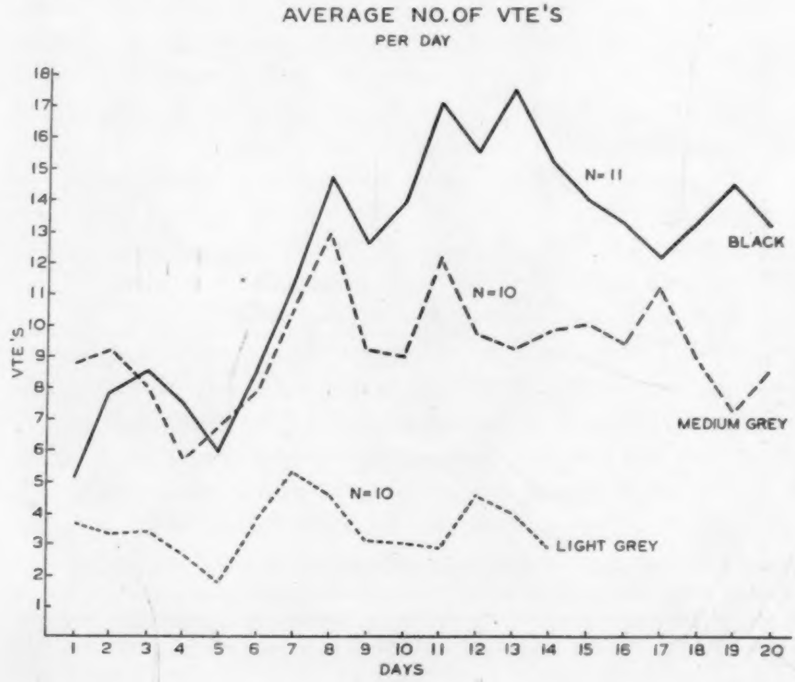

FIG. 3

Each look by a rat at one door, which was followed by a look (or a final taking-off-toward) the other door, was counted as one 'vicarious trial and error'—one VTE. For each group the average VTE's per rat in the six trials of each day are shown in Fig. 3. The white-black group exhibited the most. the white-medium gray group the next most, and the white-very-light-gray group the fewest VTE's.

Figure 4 presents the time curves. These are the times

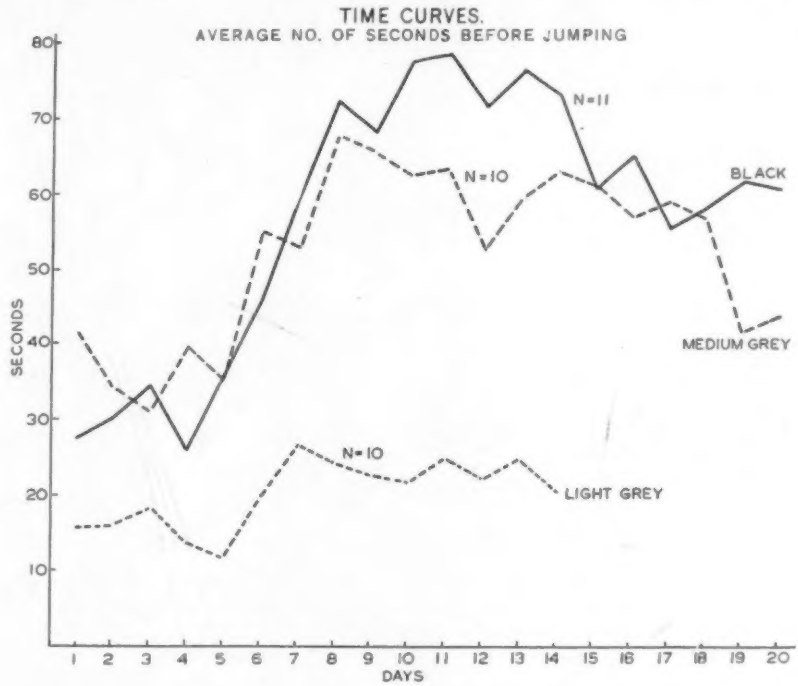

Fig. 4

from the instant the animal was put on the stand until he first took off either to the correct or to the incorrect door. It will be observed that these time curves are quite similar to the VTE curves.

Finally, in order further to emphasize the picture, curves for a couple of single individuals are presented.

Figure 5 shows the curves for an individual in the white-black group. This animal exhibited a complete and unvary-

ing left-going position habit for the first seven days. He averaged only a second or two before taking off to the left, irrespective of whether this was correct or incorrect. On the third trial of the 8th day, however, he slowed up, made one VTE, took 16 seconds, and then jumped to the right-hand side, and this was correct. From then on he began making more

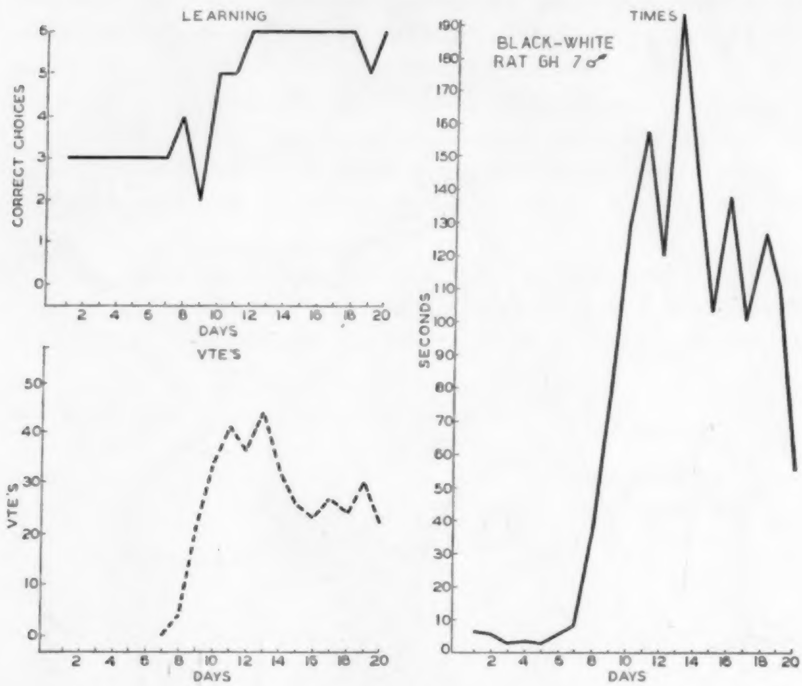

Fig. 5

and more VTE's and averaging more correct jumps even when they were to his previously avoided right-hand side.

Figure 6 shows the same three curves, for an individual in the white-medium-gray group. It will be observed that the learning did not become so complete and the VTE's and the longer times did not begin so soon and did not begin to come down again with any such certainty as was the case for the rat in the white-black group.[3]

[3] These two animals were selected since they were deemed 'typical.' It must be admitted, however, that there was considerable overlapping between the two groups.

So much for the facts; let us turn now to the theory.

The Theory

The theory (which in its present stage should not be taken too seriously) consists in the assumption that all higher organisms are in a certain *underlying* sense tropistic. This, not literally but figuratively speaking, tropistic or taxic character

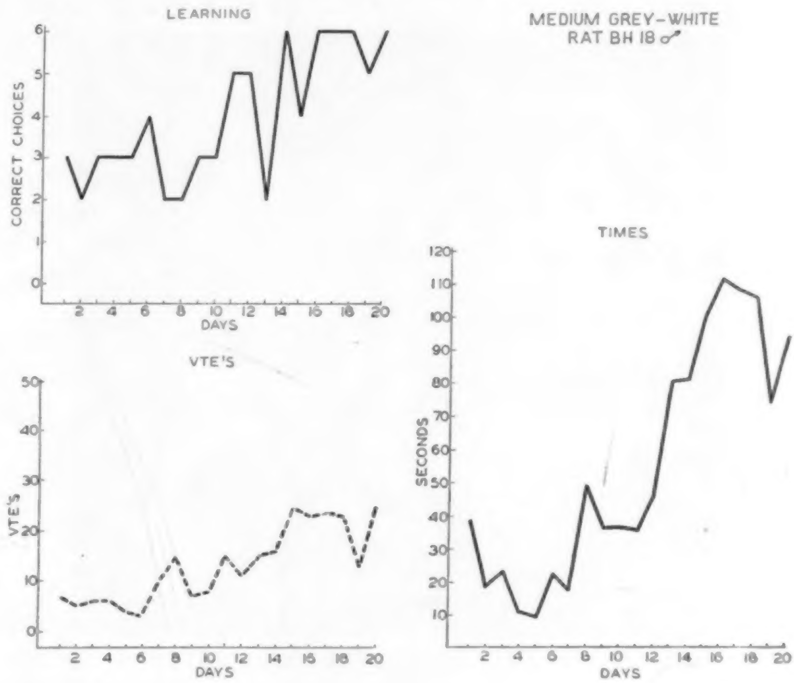

Fig. 6

of higher organisms provides, I believe, the necessary model for stating how in this experiment with rats different strengths of hunger, different goodnesses of perceptual differentiations, and different degrees of learning, combine to produce the actual amounts of correct choosing and of VTE-ing that they do. Or, to put it another way (see Fig. 7), the schematic

In particular, it is to be noted that there were one or two rats in the white-black group who never made many VTE's and yet learned quickly. Their VTE's, what there were of them, did, however, go up just before learning and then come down again.

sowbug is my present conception of what I have elsewhere (**11**) merely labelled as the f_3-function. The name 'schematic sowbug' is, of course, merely a would-be picturesque term for this essentially tropistic character which I have introduced into my f_3-function.

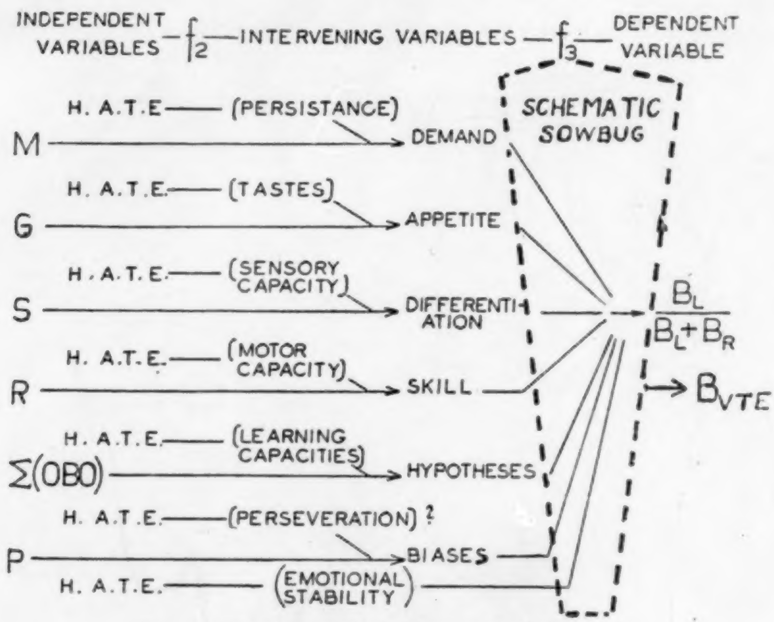

FIG. 7. The 'Schematic Sowbug' as the f_3-function.

Blum (**1**, **2**, **3**, **4**), working with simple actually tropistic animals, discovered that many of the apparent deviations from the forced-movement type of behavior to be predicted from Loeb's theory could be explained by assuming that progression velocity (forward or backward) may vary independently of orientation velocity. I have taken over this distinction between orientation and progression for my bug and then I have assumed, further, that any set of qualities, which form a discriminable dimension for the given species, can be represented conceptually as lying at different angles around the nose of such a bug. Thus, if two qualities in such a dimension are relatively near together, they will be represented as lying in the sowbug's schematic space with a narrow angle between

them; but, if the two qualities are discriminably far apart, they will be represented as having a wider angle between them. The white and the black in the present experiment I would represent, for example, as lying some 90 degrees apart in the sowbug's space, but the white and the medium gray as lying, say, only some 60 degrees apart in this same space.

Figure 8 presents a first diagram of the bug. It is shown

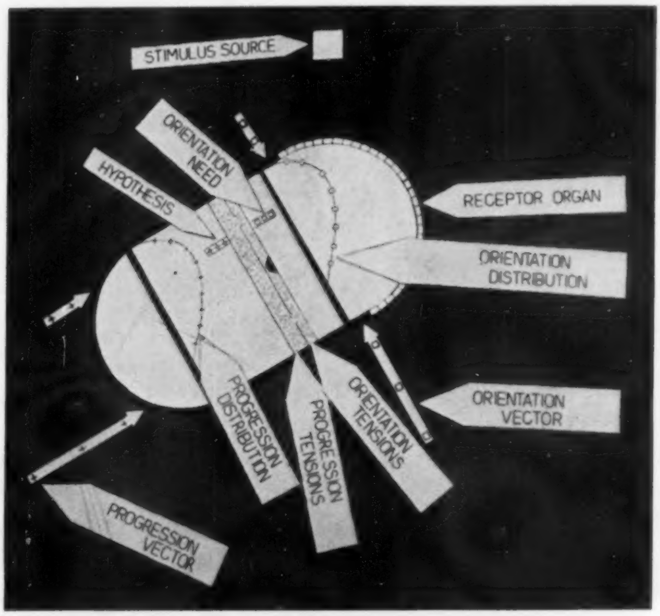

FIG. 8. The 'Schematic Sowbug.'

as lying at an angle to the parallel rays from a single stimulus source, say, white. It is bilaterally symmetrical. It has symmetrically placed pairs of motor appendages. These latter, however, are represented by vector arrows rather than as actual motor organs. The pair with small white rectangles on them, at the front just behind the head, are the orientation vectors, the pair with the plus marks on them at the rear are the progression vectors.

The curve with small rectangles labelled 'orientation distribution' indicates the relative intensities of perceptual

stimulation of the successive receptor points. These strengths of excitation are to be conceived as determined by the angles at which the rays hit plus the angular distances of the receptor points in question from the animal's median plane. The more perpendicularly a ray hits and the nearer the receptor point, which it hits, is to the nose (*i.e.*, the median plane), the stronger the excitation. When all the rays come (as in this case) from the left, the distribution is skewed and its mode lies to the left of the median plane. But when the rays come straight on, the orientation distribution becomes symmetrical. It is also to be assumed, following Loeb, that the right-hand motor appendages are activated by the left-hand receptors, and the left-hand motor appendages by the right-hand receptors. (I am assuming, that is, a positively taxic bug.) In other words, the area under the orientation distribution which is to the left of the median plane determines the length of the right-hand orientation vector, and the area under this curve which is to the right of the median plane determines the length of the left-hand orientation vector.

Further, it must be noted that although the shape of the orientation distribution is determined by the angular direction of the rays, its height is determined, rather, by a specific 'orientation need' relative to the given quality. This specific need is indicated by the little column with rectangles in it which is shown as rising up out of the upper stippled area, which latter represents the general orientation readiness or tension. This way of representing the tension or readiness as a stippled area inside the organism, the degree of stippling corresponding to the degree of the demand (derived in this case from hunger), I have adapted from Lewin (**6**). Further, it is also to be assumed that once the bug has looked directly at a given stimulus source the specific need relative to this specific quality and the corresponding resultant orientation distribution both temporarily sink.

The rear curve with the plus signs—the progression distribution—is to be conceived as following as to shape the orientation distribution. That is to say, its mode will shift following that of the orientation distribution when and if the

animal turns. The height of this progression distribution is to be conceived, however, as determined by the strength of the specific hypothesis—represented by the lower column with the plus signs on it—which hypothesis, in this case, is to the effect that the given stimulus source is good. This hypothesis in its turn is a product of the general progression tension—represented by the lower stippled area—plus specific past experience relative to this specific type of stimulus

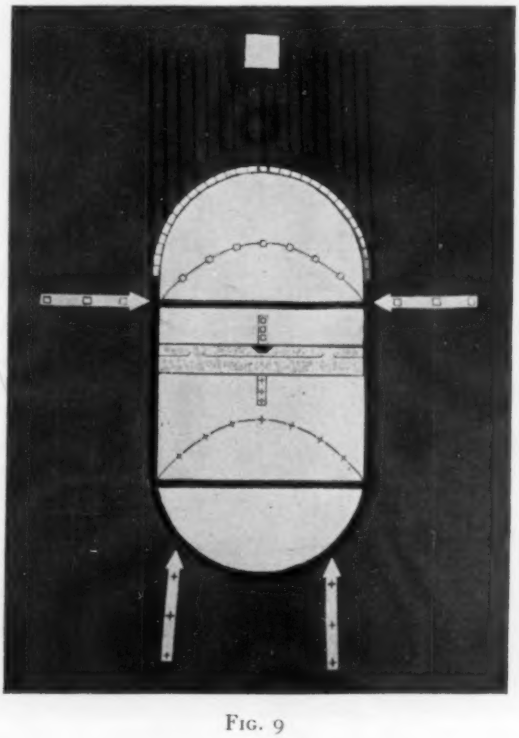

FIG. 9

source. It is such past experience which makes such a plus hypothesis ready to erupt. Note, further, that it is the right-hand and left-hand areas under the progression distribution which determine the relative lengths of the two progression vectors.

It is obvious that as here indicated the bug is in unstable equilibrium and will turn left until, as shown in Fig. 9, it will

face the stimulus, where its orientation and progression distributions will be symmetrical and, if nothing else happens, it will proceed to progress forward directly towards the stimulus source.

Now let us turn to the case of two competing stimuli, for this is the situation represented in our experiments, and the one in which VTE-ing occu: s. The next two figures show the

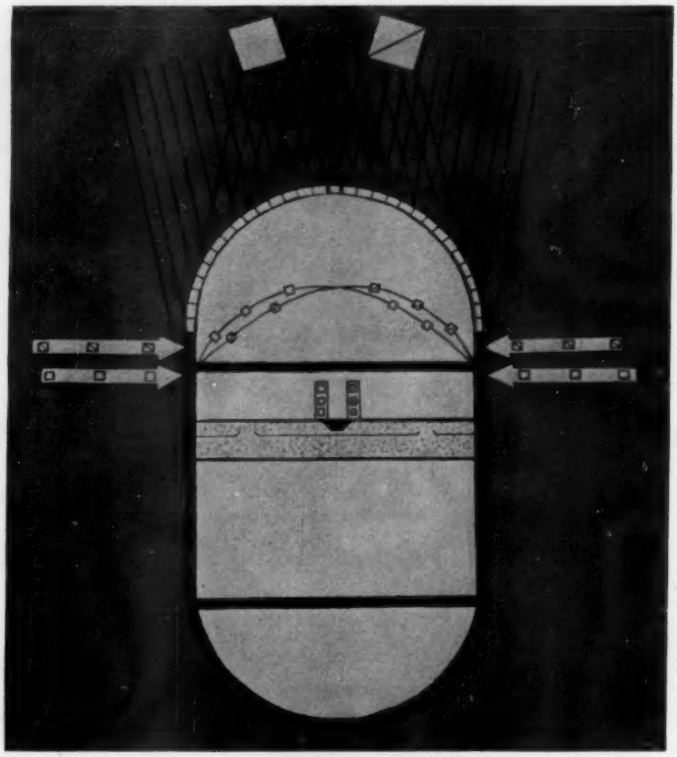

Fig. 10. Difficult discrimination.

relative sizes of the orientation vectors, first for a difficult discrimination (Fig. 10) between the two stimuli (represented in the figure by a white rectangle and a white rectangle with a diagonal line) and second for an easy discrimination (Fig. 11) between the two stimuli (represented in the figure by a white rectangle and a black rectangle). It is obvious that in this

latter case the imbalance of the orientation vectors is relatively great. Both figures, however, show the animal in the doldrums. In both cases he is like Buridan's ass between the two equidistant bundles of hay. Suppose, however, that he has just looked directly at one of the two stimuli, then our assumption is that his orientation need for that one,

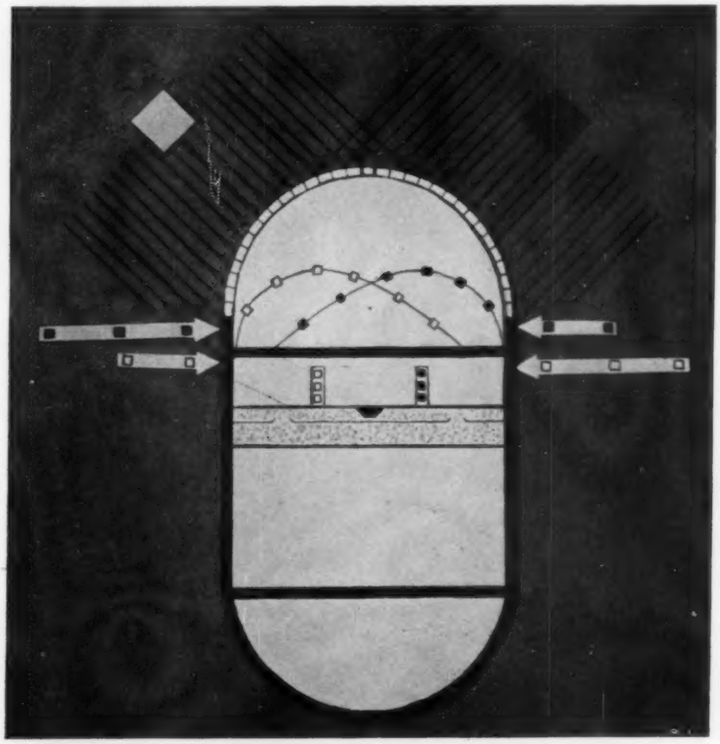

FIG. 11. Easy discrimination.

say white, will have become temporarily depressed. His white orientation distribution will have become lower and his white orientation vectors will have both become relatively short and he will swing over toward the right. But thereupon his orientation need with regard to the right-hand stimulus will, we assume, also become temporarily weaker while the one for the left-hand stimulus will recover and so he will swing back again. In short, he will VTE.

Why, however, does this tendency for VTE-ing appear sooner for the white and black choice than for the white and gray choices? As was said earlier, all the animals tended to start with initial position-habits—that is to say, initial irrelevant hypotheses. But an already established hypothesis will tend as such to interfere with VTE-ing. For, if this

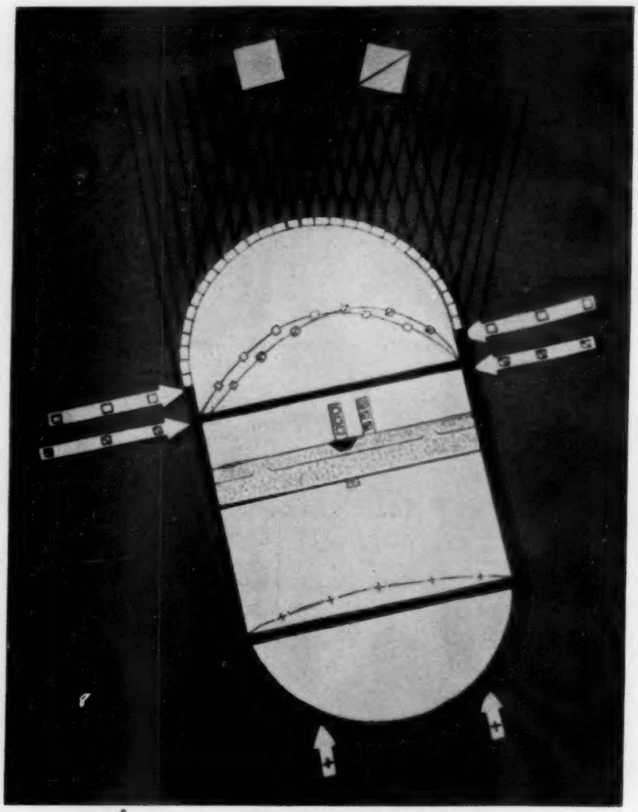

FIG. 12. Difficult discrimination.

hypothesis is strong enough, the animal will simply progress toward the given side and the imbalance in the orientation vectors relative to the brightness value of two qualities will not have a chance to get in its effects.

The next three figures indicate the relative heights of such already established initial hypotheses which would be big

enough thus to *prevent* VTE-ing. First, for a difficult discrimination (Fig. 12). It will be seen that here only a small initial irrelevant hypothesis in favor of the one side will be enough thus to prevent VTE-ing from occurring. Next consider a mediumly difficult discrimination (Fig. 13). Here

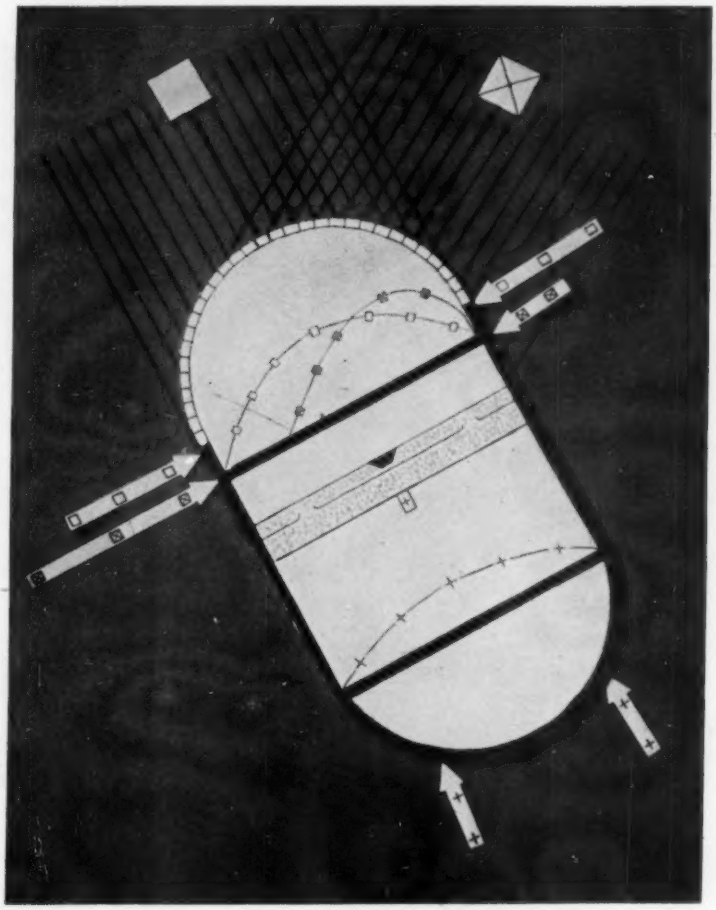

FIG. 13. Medium discrimination.

a stronger initial irrelevant hypothesis would be necessary to prevent VTE-ing. And, finally, for a still easier discrimination (Fig. 14) a still stronger initial hypothesis would be necessary to prevent VTE-ing. Hence VTE-ing starts more

readily for the white and black easy discrimination than for the more difficult white and medium gray discrimination and still more readily than for the white and very light gray very difficult discrimination. The initial irrelevant hypotheses are less likely to be strong enough to prevent VTE-ing in the case of the easier discriminations.

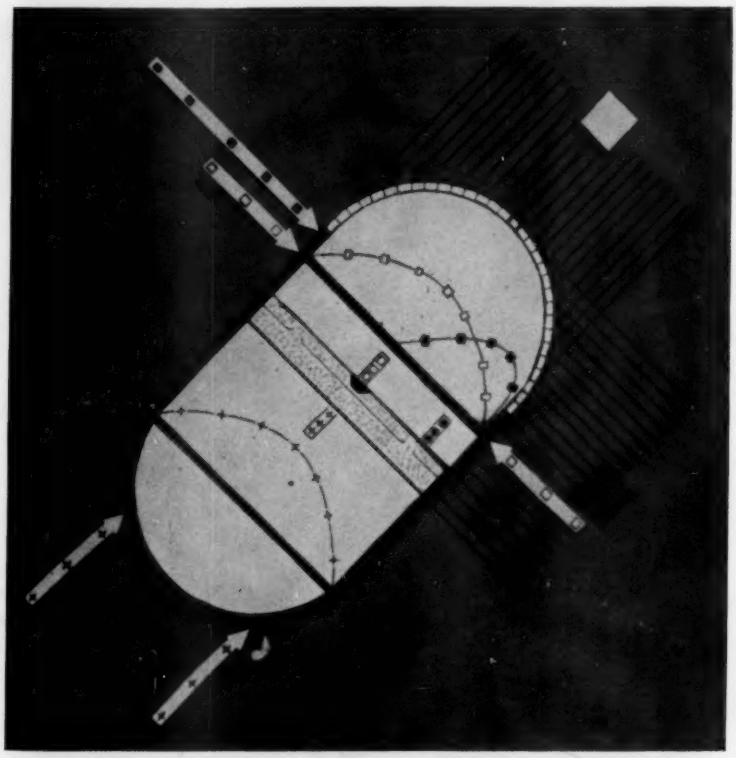

FIG. 14. Easy discrimination.

Finally, let us consider the course of learning. Figure 15 shows the situation, as I conceive it, early in learning for a white vs. medium gray discrimination. The initial irrelevant hypothesis has through lack of success been displaced and correct hypotheses have through the repetitions of success and failure begun to be set up. That is to say, a plus progression curve has started for the plus (white) stimulus source and

a minus progression has started for the minus (gray) stimulus source. There are plus forward-*pushing* vectors and minus backward-*pulling* vectors. These resultant progression vectors both plus and minus are, however, still relatively small, and VTE-ing still occurs.

But such VTE's, as soon as they appear, are, I shall assume, a help. I shall suppose, in short, that each time the

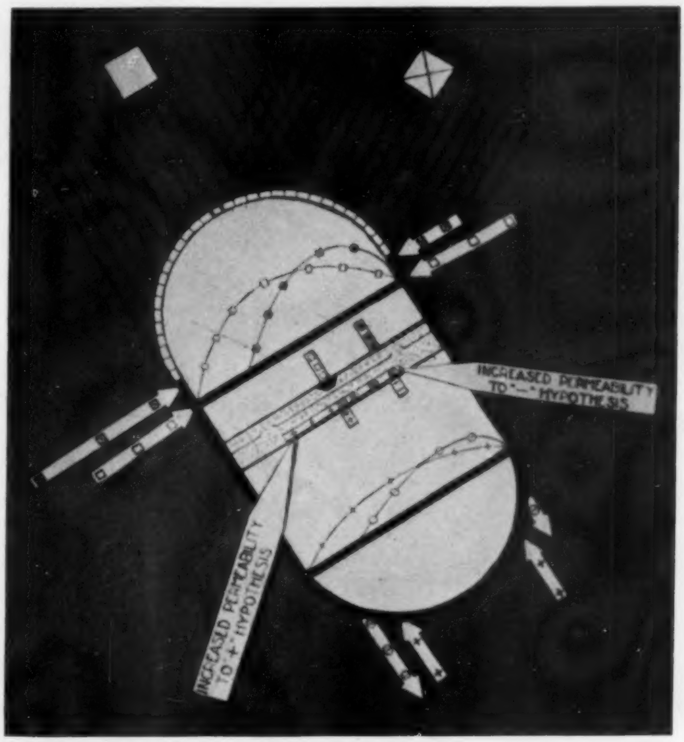

Fig. 15. Early in learning.

rat VTE's and looks at one or the other door there is some slight tendency for the effect of the correct hypotheses already forming to be reinforced. And since VTE-ing occurs more readily for the white-black group, this means that the white-black learning tends to be faster than this white-medium-gray learning. Figure 16 shows the situation later in learning.

Here the positive and negative vectors have become very strong. They have overcome the imbalance of the orientation vectors. There is practically no VTE-ing, and the bug now progresses directly towards the positive (*i.e.*, white) stimulus.

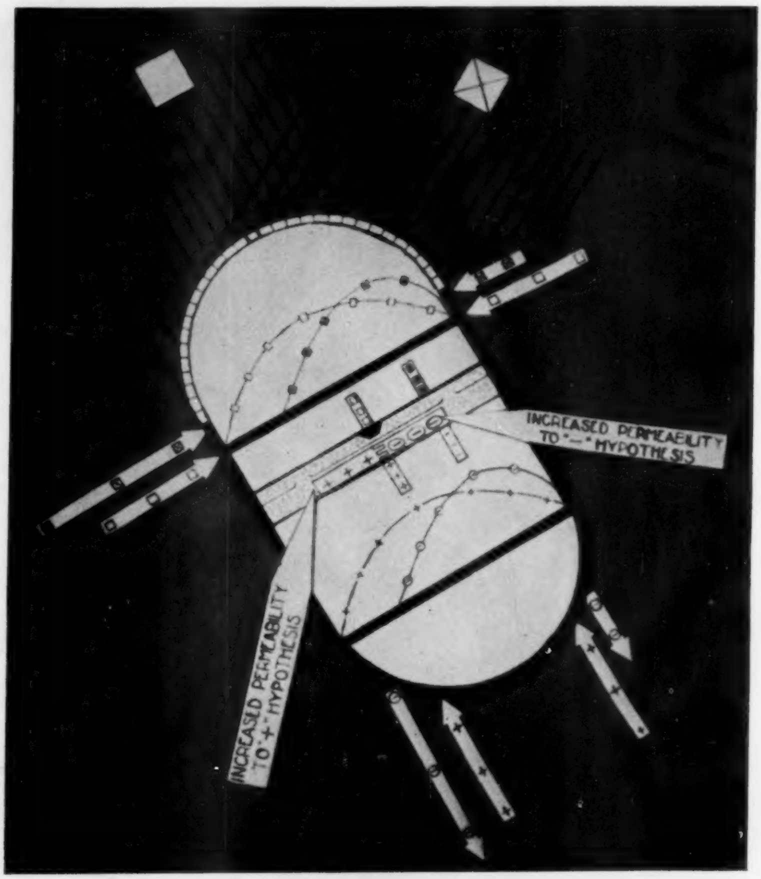

Fig. 16. Late in learning.

Another point about both Figs. 15 and 16 is very important. It will be noted that along the lower membrane of the progression tension I have tried to indicate the fact of increased permeabilities to the hypotheses which are being acquired. That is to say, as learning progresses the right-hand side of this membrane is conceived as becoming more and

more permeable to the negative hypothesis and the left-hand side of the membrane as becoming more and more permeable to the positive hypothesis. The effect of learning is thus conceptualized by increases in the respective permeabilities to the two hypotheses.[4]

One last word. This schematic sowbug, however bizarre and silly it may appear, is of course in no important sense original. All its more essential characteristics have been proposed by others. I conceive it to be patterned very much after Lewin's psychological 'person' as he draws such a person in a 'life space' (**6**). What I have done is to add to Lewin's diagram a nose and a tail and more specific receptor and motor mechanisms; I have also added the notion of spatially representing discriminable arrays of qualities as different angles in the 'life space.' And, of course, as I have already pointed out, it was from Loeb and Blum that I borrowed the notion of the manner of interconnection of these sensory and motor mechanisms according to the forced movement doctrine of tropisms. Again, it was from Spence (**8**)—though he may not like it— that the notion of the orientation and progression curves was taken. I have merely adapted Spence's generalization curves and put them inside my bug.

Finally, I wish to express my very great indebtedness to Mr. Clarke W. Crannell for his help in designing some of the actual details of the bug and for making the photographs from which the above figures have been copied.

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[4] Another and possibly better way to have conceived the bug might have been to have called the permeabilities the hypotheses and then to have called the resultant projecting columns not hypotheses, but progression needs. The height of the progression needs would then have been said to result from (*a*) the intensities of the general progression tension together with (*b*) the strengths of the corresponding hypotheses (*i.e.*, the specific degrees of permeability, respectively, to plus progressions and to minus progressions).

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SOME FACTORS IN BRIGHTNESS DISCRIMINATION

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Psychology has always leaned on the nervous system for explanation. When much less was known about its behavior, the psychologist resorted to neurological postulations to account for such phenomena, for example, as sensation and learning. With the advent, however, of the more precise electro-physiology of recent years which depicts in detail the behavior of the nerve fibre and the sense-organ, he has found it more difficult to utilize neurological information due to its complexity and to the rapidity with which it accumulates.

Various theories of nervous behavior have been built on the basis of impulse frequency. The outcome in some cases has been undue emphasis on the aspect of simple conduction, as for instance in photochemical theories of vision which deny the possibility of the neuro-retina having a part in organizing the sensory message.

Although the discharge frequency of the isolated sense-cell is dependent on intensity of stimulation, when a number of sense cells are operating together, as in the intact retina, it is improper to reason that their frequencies are always reflected unaltered to the higher centers in the nervous system. Nevertheless, it must be admitted certain types of experiments make it appear that the quantitative aspects of visual sensation are directly proportional to sense-cell activity, mediated to the cortex as if by a direct system of conductors. Despite the simple relationship between sense-cell discharge frequency and sensation in these cases, there are many known sensory end-results that require interposed quantitative modification

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of the sense-cell output for their explanation. It would seem that brightness discrimination varies all the way from being a fairly accurate reflection of the events in the sense-cell to being an entirely unexpected thing, depending upon whether the visual field is homogeneous or differentiated, and whether steady illumination or repeated flashes are used.

It has been rather convenient to construct a picture of the visual mechanism from the simple cases, ignoring the instances which do not follow the simple rule. Some investigators seem to have relied on this procedure to make their story simple enough to be understood by those who are not specialists in the study of vision. Thus it is implied that the discrepant cases belong to the brain. This procedure has led to a false simplicity in the light of the mere existence of the retina as a homologue of the spinal cord, not to mention the experimental evidence that has accumulated in late years.

Another tendency in the construction of the simple picture has been to attribute as many of the determinants of sensation as possible to the periphery, where events can be better tested. This, however, has not been consistently followed. The functional complexities required to account for the many well-known sensory effects not in line with the simple rule have not always been tentatively attributed to the retina and tested there. It is only after this has been done that any modifying activity can be safely attributed to higher centers where specific inputs are lost to experimentation in the general mass of what are conventionally thought of as associative processes.

The aim of the present discussion is to show that the retina itself, in keeping with its own properties and limits, organizes its own output into a pattern which may differ greatly from the pattern it receives. The nervous pattern has the properties of a field and cannot be predicted from the mosaic sense-cell input, but can be understood only through methods which are capable of revealing its behavior. In this connection it is to be remembered that almost all the work on sense-cells has been on single cells, and has aimed to tell what they do as individual units rather than what message is transmitted in the optic nerve with a miniature nervous system, the retina,

interposed. Furthermore, in sensory experiments it has only been possible to arrive at a notion of the photochemistry of sense-cell activity by selecting the situations in which the visual field presents the minimum of differentiation. This amounts to a selection of cases in which the retinal synaptic layers are playing their minimal observable role of organization. Thus it happens that the role of neuro-retinal organization has been overlooked, and generalization from such a selection of cases has been misleading.

The sense-cell.—The sense-cell's response to illumination consists of a series of very brief discharges ranging from a very few per second up to 130, in Limulus, the horse-shoe crab (**29**). In humans it reaches perhaps 150 to 200 per second (**28**). The only variables possible in the function of the sense-cell are those of discharge frequency, including temporal pattern, number of cells active, and particular cells involved.

Discharge frequency produced by isolated flashes is a function of intensity of stimulation. It is *increased* by raising illumination, and if the periods of illumination (flashes) are shorter than latency of response, by extending their duration, in which case intensity and duration are reciprocal. Raising illumination also shortens latency of response and increases the total number of impulses in it. Thus the relation between sense-cell discharge and the stimulus, the basis of which is attributed to sense-cell photochemistry, is simpler than we might expect the relation between sense-cell activity and *sensation* to be. But it will be seen later that even the former relation is complicated.

Pattern of discharge.—These discharges appear in various temporal groupings due to changes in the stimulus intensity. That is to say, at the onset of intense illumination there is a burst—a group of impulses at high frequency, followed later by a continued discharge at a much lower rate Under some conditions the initial burst is followed by a pause or 'silent' period before the lower rate sets in (Fig. 1). If the sudden increase in illumination from zero or from some other preexisting level is not great, a definite burst will be absent. The response will begin with a discharge rate, little if any greater

than the one to be maintained throughout the duration of the given illumination. These facts have been determined from the simple preparations of the Limulus where the action of the sense-cell can be recorded prior to any synaptic connections (**29**).

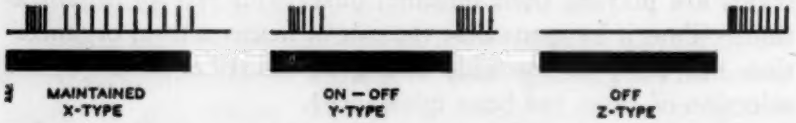

Fig. 1. This diagram shows the three types of impulse discharge from optic nerve fibres. Note the silent period after the initial burst in the maintained discharge. This is absent with weak stimulation. It is evident that the discharge of the optic nerve taken as a whole is not so simple as if it were the direct output from the sense-cells.

On and off responses.—That all sense cells do not necessarily act alike with reference to changes in illumination has been shown by Hartline (**27**) in the *Pecten* (scallop), in the eye of which there are two sense-cell layers, the cells in one discharging to the onset or increase of illumination and in the other to its cessation or sufficient reduction.

Most animal forms do not have two discrete sense-cell layers, and therefore it cannot be determined whether two or more distinct kinds of sense-cells exist in them. If they do, they indistinguishably intermingle. The discovery, however, of two functionally different sense-cells does demonstrate their possible existence elsewhere. As to what their existence might mean in photochemical terms, it is not yet certain. Where they exist, the pathways originating in these cells are either separate through the retinal layers and possibly to the brain or else their discharges affect each other, the interaction playing a part in the retinal organization of the sensory message.

On and off responses in optic nerve fibres.—In the vertebrate, the sense cell cannot be isolated. The simplest preparation is that used by Hartline (**28**) in which he isolates single active ganglion-cell fibres. In such cases, three different types of discharge are evident (X, Y, Z). Some fibres (50 per cent) discharge with a rapid burst of impulses at the onset or increase of illumination and at its reduction or cessation, being

quiescent in the meantime (Fig. 1). Others (20 per cent) fire with a burst at the onset or increase of illumination, soon quieting down to a more or less steady lower rate throughout its duration. The third (30 per cent) fire only at the cessation of substantial reduction of illumination. Thus it is that during steady illumination only 20 per cent of the fibres can be actuated at most.

The three modes of fibre response may be due to one of three causes: (1) that there are actually three essentially different types of sense-cells, or (2) that the diversity originates in the retinal layers between the sense-cell and the ganglion layers, or (3) that there are functional differences between ganglion cells. There is no decisive evidence for any one of the alternatives except in the *Pecten*.

Discharge to repeated flashes, C.F.F.—One of the phenomena most commonly studied in both visual sensation and its underlying physiology is what is known as critical flicker frequency (C.F.F.). This is measured in terms of the flash frequency which is required to produce the same sensation that is produced by continuous illumination. It is also the flash frequency required to obliterate the separate waves in the electrogram of the retina which are recorded from the eyeball and which follow the flashes at low frequency. Talbot's law states that flash frequencies can be reached at which the sensory effect is as if stimulation were continuous rather than intermittent and that the quantitative effect is as if the same amount of light were uniformly distributed in time. This was first found empirically by *sensory* experiments, but also can be deduced from a photochemical receptor theory providing the nervous part of the retina can be safely considered as essentially only a conductor system. Since, however, critical flicker frequency represents only one point in the total range of flash frequencies, broad generalization from it is not safe. *Sensory* effects vary according to the part of the range below C.F.F. which is used. It might be expected that if sub-critical frequencies were studied additional information would be gained which would lead to a different concept of the underlying mechanism of vision—this is just what has happened.

At C.F.F. (and frequencies just below) neither the *on-off* nor the purely *off* responses presumably occur. The only fibres discharging are the ones which fire throughout the duration of illumination (20 per cent of the total number). It seems, then, that it is these fibres which determine the intensity of sensation. On the other hand, certain facts gained from human flicker experiments (8) seem to be explicable in no other way than by supposing that even at C.F.F. either or both of the other responses (X and Z) also occur with some ratios between the light and the dark phases of the flicker cycle. For example, in one case the flash duration was 0.4 ms. while the dark phase was 20.1 ms., the whole cycle being 20.5 ms. (Fig. 2). This arrangement at the intensity used

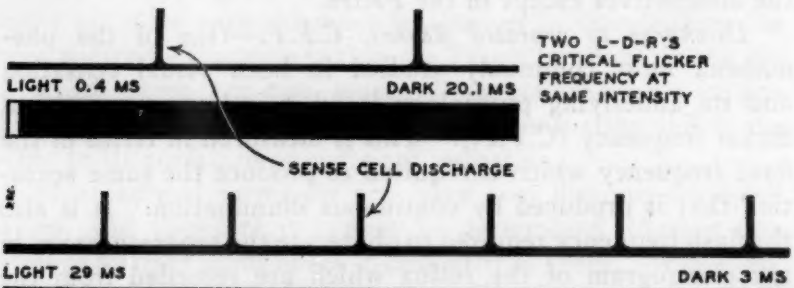

FIG. 2. These diagrams show two different distributions of light and dark in stimulus cycles which just eliminate flicker. In both cases the flash intensities are the same. The diagrams also show the possible rates at which the sense-cells discharge under the two conditions. Discharge from the sense-cell population as-a-whole from any uniformly stimulated retinal area possibly operates to mask slight irregularities in discharge rate of individual sense-cells. Thus when unevenly spaced discharges occur, they are not significant in the output of the group whose elements are probably not all firing at precisely the same rate anyhow.

just produced fusion (C.F.F.). With the same intensity and same set-up, a 29.0 ms. flash in a 32 ms. cycle just produced fusion, the dark interval being only 3.0 ms. It is evident that the two situations (light-dark ratios) are quite opposite.

In the latter case, if the sense-cell discharge rate is 200 p.s. there will not be more than 6 impulses during the life of the flash, though it is more likely that the rate is 100 p.s., allowing

for only 3 impulses per flash. When the 3.0 ms. dark interval is lengthened, flicker appears. It is not apparent whether or not the distribution of the impulses is thereby made irregular, thus causing the flicker.

In the former case, the flash being 0.4 ms. long and the dark interval 20.1 ms., the discharge rate is supposedly lower. Since the flashes although very short are still much above threshold, we shall assume they each produce at least one impulse. There are perhaps only two or three sense-cell impulses per stimulus cycle. It is hard to believe that if the dark interval were lengthened slightly the impulses would be materially altered in distribution. If there is one impulse for stimulus cycle in the second situation, the sensory effect ought to be about one-sixth to one-third as great as in the other case, assuming sensory intensity directly dependent on discharge frequency, but we know that the first is only 1/46 as bright as the other (Talbot's law).

Furthermore, with either one of these rates it is not apparent how a slight change which we know introduces flicker could bring about an irregularity (pulsation) of discharge to account for flicker. In a following section, it will be shown that flicker and flash rates do not go hand in hand, so the problem as generally put is possibly false in the first place.

If a 0.4 ms. flash produces a physiological effect resulting in sensory uniformity over the 20.1 ms. dark interval, and the 29.0 ms. flash fails to do the same when the dark interval is lengthened beyond 3.0 ms., then some different process must occur in the two cases. The institution of a definite *off* discharge must begin if the 3.0 ms. interval is lengthened. The flash duration is relatively long and the dark interval probably long enough to allow for an *off* response of threshold value before being inhibited by the next flash. Very short (12 ms.) dark intervals are sufficient to produce recordable *off* responses on the optic cortex, where the practical threshold values for response are much greater than the ones necessary to produce sensory effects (**6**).

Discharge at rates below C.F.F.—What occurs when flash frequencies below the C.F.F. are used? Certain facts show

that as frequencies fall farther and farther below C.F.F. the results become quantitatively different. The phenomenon first reported by Brücke (1864) and studied more in detail recently (10) consists in a shift from the Talbot effect to a state in which intermittent light is more effective in terms of sensation than is continuous light (Fig. 3). The maximum of

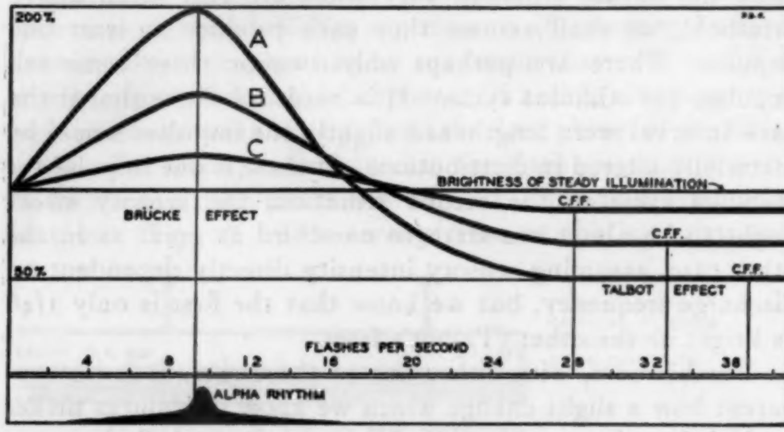

FIG. 3. These curves show the change in the apparent brightness of a series of flashes as the rate is changed from C.F.F. down to a very few per second and then to continuity; the relation between the Talbot and Brücke Effects. Curve *A* represents the outcome with flashes and dark intervals of equal length (L.D.R. 1/1); Curve *B*, L.D.R. 7/2; Curve *C* 8/1. Curve *A*, for example, in which the flashes and dark intervals are equal in length, indicates that at C.F.F. the test spot appears to be ½ as bright as the standard. For actual measurement, the physical intensity of the flashes is increased till the test and standard spots appear equal. In this case the flashes had to be twice as bright as the steady illumination in the standard. As flash rate is reduced, the average brightness of the flickering test spot appears greater and greater till it equals and then surpasses the steady standard. In the region of 8–10 flashes per second the maximum is reached and it is twice as bright as the standard. This is about the rate of the alpha rhythm of the electroencephalogram (see text for the relation).

this effect is reached with flash frequencies of 8–10 per second. This frequency might be slow enough for the *on-off* fibres to function. If with slower and slower flash frequencies more and more of the *on-off* fibres are brought into activity, then the enhanced effect might be due to increase in total number of fibres active as well as to the commonly postulated increase in sense-cell discharge frequency. This assumption might be checked by examining the electrogram of the retina. If the

supposition is true, Brücke effect will show up in the eye, at the proper low flash rate. If this happens, the sensory outcome may or may not be determined directly by photochemical behavior without distortion by the neural activity in the intervening layers of the retina. Since, however, the flash rate necessary to produce the maximal of sensory effect is the same (8–10 p.s.) for all intensities and all practical ratios between light and dark phases of the stimulus cycle, it would seem that some factor external to the sense-cell must determine it. That this mechanism is even *external* to the eye is made probable by studies on the behavior of the optic cortex of the rabbit (**12**, **5**, **10**). With the eye removed, the optic nerve in the rabbit, when stimulated by slowly repeated random shocks, produced cortical responses which fluctuated in amplitude from trial to trial. If, however, a second stimulus was given at about one-fifth second after the first, the response was a maximum in both cases, indicating a cortical rhythm. This was attributed to the same mechanism as the alpha rhythm in this animal, which has a one-fifth second period. The rate of the alpha rhythm in man is of the order of 8–10 per second, and a facilitative effect on incoming stimuli at a corresponding rate might be expected. It is highly plausible that the Brücke effect is evidence of it.

Discrepancy between flash rate and flicker.—It has been assumed without question that up to the point of C.F.F., subjective flicker follows flash frequency regardless of the latter's actual value. It has been taken for granted that just before flicker disappears and the C.F.F. is reached, the seen fluctuations (flicker) still occur at the rate of the flashes themselves. In actual experiments, the flash rate for fusion varies from 3 to more than 60 per second, which would mean that the last vestige of flicker with faint flashes has a very slow rate (3 p.s.), while the last vestige with intense light and a high flash rate (60 p.s.) can be seen as a very rapid fluctuation, supposedly 60 per second. That *this is not the case* has been shown (**9**). The following is one of several results by which this fact has been demonstrated. With flash rate remaining constant (for example, at about 4 per second) the flicker rate

will *increase* when the flash intensity requiring a C.F.F. of 22 per second is reduced to near threshold intensity where the C.F.F. is about 4 per second. This with the other results indicates that vestigial flicker is about the same rate regardless of flash frequency. If this be so, then it would seem that certain factors intrinsic to the neural mechanism itself determine C.F.F. This becomes obvious in some situations while in others the repetitive sense-cell discharge and these temporal properties of synaptic layers of the retina are so near alike that sense-cell behavior determines the quantitative aspects of sensation quite directly; the retina in these latter situations seems to be only a simple conductor system. As it happens, however, the concentration of study on cases of this latter kind to the exclusion of others has been misleading. The fact that synchronization of discharge, in the optic nerve, comparable to the impressed rhythm from intermittent stimulation, develops spontaneously (**1**) is also contributory evidence in favor of the optic nerve discharge pattern being in reality an intrinsic function of the neural apparatus of the retina. In such a case, the discharge would be quite similar in principle to the intrinsic rhythm of the motoneuron (**16**).

Retinal elements and the components of the retinogram.—The electrical record obtained when leads are placed on the eyeball is known as the retinogram. The first studies using it were made many years ago. Lately attention has again turned to this means of studying retinal phenomena.

The retinogram obtained when a light is flashed on and left for a matter of seconds consists in the parts shown in Fig. 5. The *a* wave is the initial negative wave, the *b* the positive wave which follows it, the *c* wave the long gradual drift following the *b* wave, and the *d* wave is the variable shaped deflection caused by turning the light off.

The retinogram has been analyzed into three components, P_I, P_{II}, and P_{III} (**25, 33**). P_I is the *c* wave, and is absent with brief flashes. The origin of components P_{II} and P_{III} can best be understood if the structure of the retina is considered.

The connections of the neural elements of the retina are of two kinds, parallel and in series (Fig. 4). Of the latter type

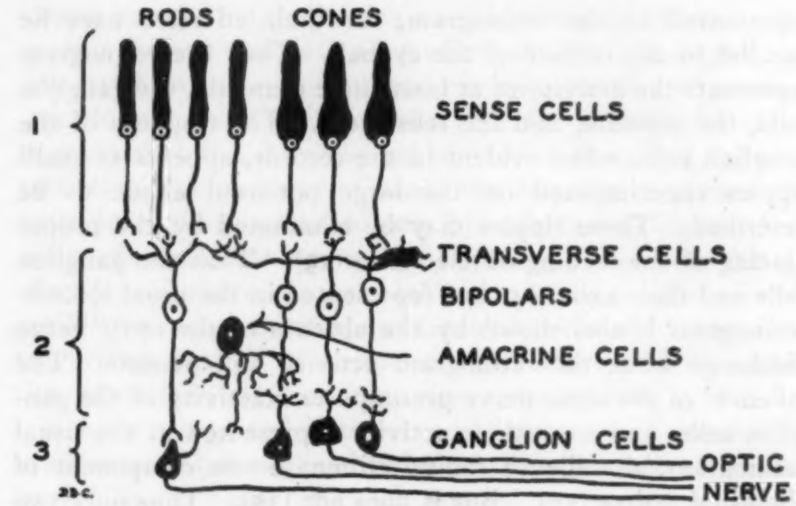

Fig. 4. A diagrammatic cross-section of the retina showing the serial and parallel connections, as described in the text. Only those potentials which are effective in a direction perpendicular to the surface of the eyeball appear in the retinogram (see text for the analysis).

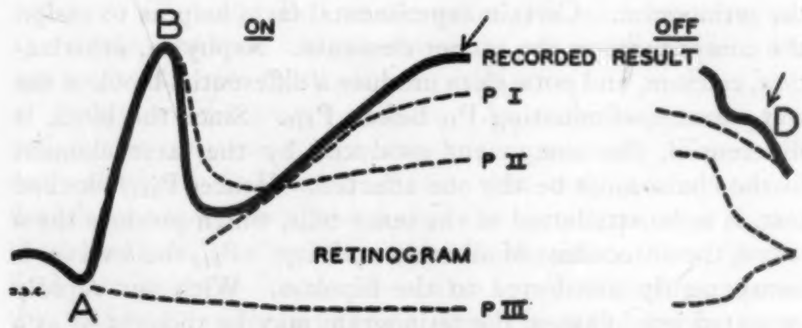

Fig. 5. These curves show the *on* and *off* responses of the retinogram, the massed effect of the activity of certain of the elements in the retina. The components which have been analyzed out by Granit are also indicated. The activity of some of the retinal elements does not appear in the retinogram due to the geometry involved in recording from the surface of the spherical eye-ball.

are those between the sense cells, the bipolars, and the ganglion cells, in the order named. The transverse cells and the amacrines make predominantly cross or parallel connections between the members of the serial chain. Responses of these latter cells are not found in the recorded retinal potentials

represented in the retinogram, for their effective axes lie parallel to the surface of the eyeball. Thus the retinogram represents the activity of at least three elements, the ganglion cells, the bipolars, and the sense-cells. The response of the ganglion cells, when evident in the records, appears as small ripples superimposed on the large potential about to be described. These ripples may be eliminated by the proper placing of the leading-off electrode (**19**). That the ganglion cells and their axons are not represented in the usual electroretinogram is also shown by the absence of the optic nerve discharge when the retinogram activity still persists. The 'silence' of the optic nerve presupposes inactivity of the ganglion cells, and were their activity represented in the usual retinogram, the silence should eliminate one component of the usual retinogram. But it does not (**35**). Thus only two elements give rise to the retinogram as conventionally recorded.

There are two components, P_{II} and P_{III}, to account for, and there are two retinal elements left whose activity produces the retinogram. Certain experimental facts help us to assign the components to the proper elements. Asphyxia, etherization, calcium, and potassium produce a differential block of the components, eliminating P_{II} before P_{III}. Since the block is differential, the component produced by the later element in the chain must be the one affected. Hence, P_{III}, blocked last, is to be attributed to the sense cells, which produce the *a* wave, the antecedent of all other activity. P_{II}, the *b* wave, is consequently attributed to the bipolars. With our rapidly repeated brief flashes, the retinogram may be thought of as a series of *b* waves, and thus we are dealing with the activity of the bipolars.

Granit's interpretation of the retinogram.—Although the analysis of the retinal potential into components P_I, P_{II}, and P_{III} is for the most part Granit's development, he does not assign the components to the activity of specific retinal elements. Granit was influenced rather by the fact that the *off* effect was strongly reminiscent of the post-excitation release of impulses from central neurones. In reflex studies this is

known as rebound, indicating a release from inhibition. His records showed that activation of negative P_{III} (a large negative notch in the *off* effect) was correlated with a temporary cessation in optic nerve discharge (**26**). Consequently he suggests that P_{III} is connected with inhibition, and P_{II} with excitation. According to Granit, the demonstration that the optic nerve fibres activated only by cessation of illumination are made quiescent upon superimposing a flash on the *off* effect (**29**), is also evidence of inhibition by P_{III}. Not all eyes exhibit the negative notch, and Granit has classified them into two groups, the 'E' or excitation retina and the 'I' or inhibition retina (cat, rat; and frog, pigeon, respectively).

On the other hand, both *on* and *off* responses, quite similar in appearance but more or less independent, have been demonstrated in the optic cortex, showing that they are similarly transmitted to the cortex. This fact suggests that inhibition is not a gross affair set up in one phase of the light-dark cycle and not the other (**6**).

It remains to be seen whether the assignment of the processes (P_{III} and P_{II}) to the sense-cells and bipolars, respectively, and their assignment to the processes of inhibition and excitation, are totally incompatible.

Irregular retinogram responses.—At some flash rates, the retinogram response (the *b* wave) is occasionally elicited by only alternate flashes. The fact that only every second flash is responded to suggests that the system requires longer than the flash interval to recover each time. Now, if the flash rate is slowed down somewhat, a small response occurs to the flashes formerly producing none; in other words, to a series of flashes equal in every way, the responses alternate in size. This could be due to the fact that the flash interval is too short for some functional units and not others. If the flash rate is made still slower, all responses become equal, but smaller than in the other two cases. This phenomenon showed up clearly not only in some of the records of Granit and Therman (**26**) but was seen also by Fry and Bartley (unpublished) and again by Bartley (**9**). If, as suggested, the functional unit requires more time than allowed by certain

flash frequencies to respond, reactivation of the eye by successive flashes would require the alternate functioning of parallel units serving the same retinal areas.

Change of retinogram lag with flash frequency.—Alternation of response is also indicated by the implicit time of cortical response to photic stimulation of the retina. Implicit time is a measure of latency taken as the interval between the beginning of a flash and the peak of the response, whether of the *b* wave in the retinogram or the response of the optic cortex. It was first employed for the latter case due to the inability to measure the onset of the wave accurately (2). As the flash rate is increased, cortical implicit time lengthens up to a certain point at which a rather sudden reversal takes place (Fig. 6). This reversal point is at about one-half the rate of

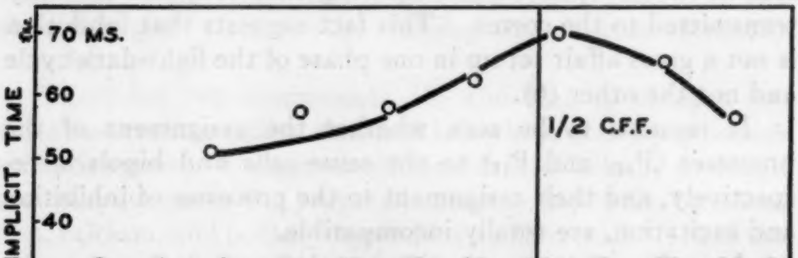

Fig. 6. This curve shows the change in implicit time (see text) with flash frequency. The implicit time increases up to about ½ of the expected C.F.F. and then shortens to the original value, due presumably to the arrival at a state in which the activated units alternate in their response to successive flashes.

the expected C.F.F. Although cortical responses are only indirect measurements of what happens in the eye, it has been shown that for some purposes they follow the features of the retinogram (2); and this is such a case. The reversal suggests that as frequency is increased the implicit time becomes so long that the same group of elements cannot respond to each flash. The lengthening of latency as successive stimuli encroach on the relatively refractory phase is not uncommon in nerve physiology, and here is apparently a case of it. By a

process of chance reorganization, some elements come to respond to one flash and others to the next. This would allow a return to longer intervals between reactivations of individual responding elements, and thus to shorter implicit times even down to the original values (7).

Latency of retinogram.—When the flash rate is still slow enough for the individual flashes to be represented in sensation by fluctuations, the bipolars receive grouped impulses (pulsations) from the sense-cells. It is not certain whether the bipolars likewise discharge repetitively to each flash or whether the *b* wave represents but a single prolonged discharge from each cell. The latency of the *b* wave, however, in some cases is greater than the interval between the origins of the flashes. This fact represents a surprising lag, one which we can hardly attribute to the sense cells. If it were due to them it would mean that they took longer to change their rate of discharge as light and darkness alternated than the time represented between any two discharges in a pulsation. This is not impossible to conceive, but it is more probable that the lag is due to more slowly responding units, which in this case must be bipolars themselves. Since the latency is greater than the flash interval, it suggests that no one bipolar cell is able to respond to each flash; but that, in order that each flash elicit a response, bipolars serving a given retinal area must take turns. The evidence here is not conclusive but it does fit in with the other facts already pointed out suggesting the same thing.

Spatial interaction.—Various factors have been used to account for the deviations of brightness discrimination from the predictions of sense-cell studies. Investigators not interested in such deviations have recognized them as parameters external to the photochemical processes and not modifying the essential nature of the visual process. Therefore, they have been excluded as improper matters to consider in the study of brightness discrimination. Other investigators have looked upon such factors as integral parts of the fundamental mechanism and attempted to measure and identify them.

Sensory phenomena resulting from stimulation with a differentiated pattern reveal the need of recognizing fundamental processes other than the photochemical activities of the sense-cells. The commonest laboratory example is brightness discrimination when the surroundings of the test field are illuminated. The surroundings may include the whole visual field or they may occupy but a restricted area such as an annulus. Such patterns have been frequently used in experiments on visual acuity, brightness discrimination in stationary fields, and in brightness discrimination studied by flicker methods.

The quantitative aspects of brightness discrimination under these conditions have not yet been predicted by photochemical theories. In order to account for them, five different factors have been used either singly or in combination: (1) Neural interaction in the retina. (2) Interaction at the thalamic level. (3) Interaction at the cortical level. (4) Scattered light in the eye. (5) A hypothetical chemical (the V substance) diffused in the retina.

An illuminated field surrounding a test object alters the absolute values obtained without it. The alteration proceeds in one of two directions, depending upon the size and intensity of the field and whether it is strictly adjacent to the test object or not. For example, maximum visual acuity exists when the surrounding field is adjacent to the test object and has the same brightness. Surroundings brighter than the test object progressively decrease visual acuity when their size is increased. When dimmer they progressively increase the acuity as size increases. Their size is indifferent when they are the same brightness as the test object (**17**).

The lowest difference-limens are obtained when the surrounding field is the same intensity as the test object. In such experiments Fry and Bartley (**21**) found that the *distance* of the *border of the surroundings* away from the test object was the critical factor, *rather than the area* of the surrounding field (**13**).

A freely diffusible chemical (the V substance) in the retina, which is developed in proportion to the decomposition prod-

ucts of light in the eye-as-a-whole, has been used to explain some of the effects of large surroundings whose visual angle lies clearly outside of the known range of neural interaction in human experiments. Most experiments place the limit of retinal neural interaction at three degrees, although the border effect mentioned above clearly extends to four degrees. Since large surroundings extending to at least twenty degrees are effective in determining both visual acuity and the plateau or drop of C.F.F. at high intensities, the idea of some kind of a chemical intensifier has been developed to account for this action (**14**, **17**, **34**). That neural interaction beyond four degrees takes place *in the eye* is given support by Adrian and Matthews in their experiments on the excised eel's eye. The V substance postulation is an alternative to making use of thalamic and cortical levels of activity, or to actually proving widespread retino-neural interaction.

In critical flicker experiments, a higher C.F.F. is obtained with a steady surrounding field equal to the Talbot brightness of the flickering field, than with no surrounding illumination (**22**, **23**, **24**, **32**). As the intensity of the surroundings increases from zero, C.F.F. is raised until a critical value is reached, when it again falls.

Summation and inhibition were invoked by several investigators to account for the effects (**24**), but they seem best accounted for by assuming that the steadily stimulated area depresses the activity in the flickering area; below the critical intensity, it depresses the *weak* phases of the activity and *increases* C.F.F.; above the critical level it depresses the *strong* phases also and thus *lowers* the C.F.F. (**21**).

Large areas require weaker stimulation to elicit threshold response than small ones. This can be accounted for by the assumption that in activating the minimal effective number of elements, fewer elements per unit area are stimulated. This result is to be expected, due to the heterogeneity of excitation thresholds among the individual elements in any unit area. It is admitted that this constancy of the total number of elements required fails for both very small and very large fields; since very small fields require intense light, certain

elements are probably excited to respond at well above threshold frequencies. It is thought by Wald that the threshold in such cases very likely corresponds to a constant over-all *frequency* rather than to a constant *number* of active elements. In very large fields the number of elements may be expected to rise.

It has been declared that the retina is a mosaic functionally and that the effects obtained as the area of retinal stimulation is increased are not due to nervous interaction (**36**). According to this view, not only is the retina a mosaic but the pathways leading from it are postulated as separate all the way to the cortex and possibly even including it. Interaction is regarded as unnecessary to explain the empirical data. The denial of interaction, at least in the cortex, is impossible. There seems to be a need for integration somewhere to account for the unity of experience. If interaction is reserved for the cortex alone and urged as unnecessary below it, the view seems to make the cortex a mechanism or thing of entirely different order than any other part of the nervous system. It would seem to be a 'little man' capable of taking a group of things which had to remain separate until arrival, and forming its own unity pattern from them.

It is to be remembered not only that there are complicated synaptic layers in the retina and in the thalamus, but that there is evidence for interaction in them. When Adrian and Matthews obtained their areal effects they were using excised eye-nerve preparations devoid of both brain stem and cortex. Latency of optic nerve response was shortened in such preparations when several areas were simultaneously stimulated. Bishop (**12**) obtained shortened latency of first response in cortical records to increased intensity of stimulation of the optic nerve. The response measured was the first wave of the cortical record reflecting what must have happened in the lateral geniculates. Increase in stimulus strength was equivalent to adding more optic fibres or to increasing the area in experiments on retinal stimulation by light. Repetitive discharge was not involved, so frequency of discharge in the separate elements of a mosaic could not have accounted for the shortening of the geniculate latency.

If the notion of a simple mosaic of the retina were true, then the retinal synaptic layers would be nothing but straight line conductors, and sense-cell activity should always be conducted through them and appear as an optic nerve discharge. Therman (**35**) has shown that the retina can be decidedly active when the optic nerve discharge is virtually if not actually absent. There seems to be no reason for not believing that the retinal synaptic layers behave in principle like a *bona fide* nervous organization, characteristics of which have been studied in other parts of the body.

Contours.—Contours could never be seen if the optico-physiological pattern of activity had to copy the stimulus. When an external object is visually perceived, its borders are seen as being distinct in spite of the fact that the optical system of the normal eye is chromatically and spherically aberrant, and that the retinal image of the object has a more or less poorly defined border due to the unavoidable irradiation of light. It happens, then, that contours are seen under some conditions where they do not physically exist in the physical stimulus, and fail to appear where they would be expected (**18**, **37**). Various neural processes have been postulated to translate the graded pattern of stimulation on the retina into one with abrupt transitions copying with some closeness the stimulus itself. Such processes as 'border-contrast' have been used, but up to the present they have not been put into concrete form. The failure to see contours where they exist has likewise not been accounted for. Knowledge, however, of nervous organization seems to be progressing in a direction indicating such a solution will lie in its domain.

The fact that five-degree patches at threshold in Wald's experiments have poorer contours, if any at all, than those of one degree is not evidence against spatial interaction. On the contrary, his density concept helps explain the phenomenon only if interaction is somewhere assumed. As areas are increased and the excited population of elements per unit area becomes less and less, the expected threshold number of elements, as he says, must increase. This is not due to 'the difficulty of distinguishing a very low density of active ele-

ments against a persistent background,' but due to the fact that interaction cannot so well take place between active elements becoming more remote from each other. A certain minimum number per unit area must be active for the scantiest semblance of *border processes* to occur. It must be remembered that all interaction is not *summative;* some of it is integrative. Adrian and Matthews' experiments show that at least some integration can take place in the *retina* itself. Wald's results, far from being such convincing evidence against interaction that his interpretation can dispense with it, fit in well with the interaction concept, and in fact are meaningless without its existence somewhere.

Though a possible V substance may account for certain enhancement phenomena, general in nature, it is not as plausible an explanation for the seeing of *contours* as is some strictly neural mechanism. If some sense-cells were discharging at a low frequency and others lying successively farther and farther away were discharging at progressively faster rates, the V substance would enhance the rates of them all, and possibly raise the general brightness level. Even if enhancement were differential and the higher frequencies were enhanced more than the lower ones, the spatial gradient would not become steep enough to account for contour. Some neural mechanism involving inhibition as well as facilitation must take a hand in the matter if we are to think of the functional pattern somewhere in the pathway as possessing sharp gradients or in some sense copying the stimulus.

Summary

We have seen that no simple relation exists between sense-cell discharge and the quantitative aspects of ultimate sensation. (1) There are three distinct types of discharge in the optic nerve fibres, due either to an original difference in the behavior of vertebrate sense-cells or to revamping of their activity in the retinal synaptic layers. (2) It seems that the known functional units in the retina cannot act as fast as would be required for the recognized ability of the eye-as-a-whole to respond to flashes of all rates. Consequently,

alternate activity of such units is required. That such alternate activity takes place is evident from the fact of irregular size of response to uniform flashes; of latency that is greater than flash interval; and of the nature of the change in latency as flash frequency is increased. (3) It has been shown that flicker rate does not keep pace with flash rate. (4) Brightness discrimination in a flash series is distorted by an intrinsic rhythm in the central nervous system. (5) Contours may or may not occur in sensation when they exist in the stimulus. This, and the fact that the eye is characterized by refractive imperfections precluding sharp images, necessitate the operation of neural influences to account for known results. (6) And the spatial arrangement of contours or borders influences the level of the limens for other contours or the parts of the visual fields they bound, and is not predicted by sense-cell photochemistry.

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THE CONCEPT OF THE INDIVIDUAL

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Psychology is concerned with the study of a certain type of activity of a human or animal organism. Both the organism and its acts are objects of study, for neither can be completely understood except in terms of the other. These acts cannot be adequately explained except in terms of the organism, and any knowledge of its reactive nature must be inferentially derived from a study of its behavior.

The psychological individual.—An organism is a complex object that is the subject of study by such sciences as anatomy, physiology, zoology, and psychology. The term "psychological individual" is here used to denote the organized totality of those features of the organism upon which its psychological activity is contingent and in terms of which that activity must be explained. It denotes the reactive nature of the organism as reflected in and revealed through its psychological behavior. It denotes all of those features of the organism that possess a psychological significance as contrasted with those features that constitute the subject matter of the other sciences.

The existence, nature, and locus of these organic conditions cannot be directly observed and specified. They must be conceptually apprehended and defined. The individual is thus a conceptual construct whose nature must be defined in terms of such categories as abilities, traits, attitudes, motives, etc. This "psychological individual" is not a new concept. It is but a new term for an old concept that has long been represented in our standard texts by such terms as "the self" and "personality."

Up to the beginning of this century, the individual was termed the ego, or the self, according to the point of view from which it was regarded. When regarded as the subject of its acts, the individual was called the ego, or that which knows,

feels, decides, and acts on the basis of these decisions. On the other hand, the individual was called a self when it was regarded as an object of knowledge.

This individual cognitively reacts to the external world of objects, but it can also possess some knowledge of itself. The individual can thus be both the subject and the object of its own cognitive activities, *i.e.*, it is an ego that knows itself. This fact of self consciousness was regarded as one of the important characteristics of the individual in the early treatments of the self.

An organism is continually active throughout life. Its successive acts are not discrete and unrelated events, for they are distinctions within a temporal continuum, and they are functionally interrelated in several ways. (1) Present activities are contingent upon the previous activities of the organism, and they in turn materially influence the character of all of its subsequent activity. Without this contingent relation between past and present, an organism cannot learn. Destroy this relation, and all that has been learned throughout life will immediately disappear, and the reactive nature of the organism will be so altered as to be unrecognizable. (2) A correlated fact is the memory connection between past and present. All the experiences of a lifetime are so interrelated that given the proper conditions each can be recalled upon some subsequent occasion. These functional interrelations are examples of what is termed integrative unity.

It is obvious that this phenomenon of integrative unity is one of the basic and important concepts of psychology, and that it is a function of, and must find its explanation in terms of, the nature of the organism. Integrative unity thus must be regarded as one of the fundamental characteristics of what we have termed the psychological individual, and we find that this attribute of the individual has been recognized and emphasized in the early literature.

Inasmuch as the self is a product of the integrative process, the nature of the self must necessarily change and develop with the accretion of experience. We thus have the correlative fact that the self exhibits growth and development throughout life.

While the treatments of the self naturally differed to some extent with the author, yet the various chapters on the self were essentially alike in respect to choice of topics. Practically all authors were accustomed to discuss the nature of the self under the three rubrics of self consciousness, integrative unity, and growth and development.

The more recent treatment of the individual has been materially influenced by the gradual trend toward objective observation—a movement that found its culmination in the doctrine of behaviorism. As a consequence the individual came to be studied and described more and more in terms of objectively observable behavior. Three results of this movement may be briefly mentioned.

1. The field of psychology was extended to include the animal world, for psychologists began to study psychological behavior wherever found. This extension of the field necessarily expanded the concept of the individual, for infants and animals must be regarded as psychological individuals in so far as they exhibit behavior of a psychological character.

2. The ego and self conscious attributes of the self were discarded. The behaviorists assumed that a knowledge of the existence of this attribute of self consciousness is exclusively derived through a process of self observation, and hence that its inclusion is not consonant with an objective treatment of the individual.

We may note in passing that the validity of this assumption may be seriously questioned, for the existence of this attribute is frequently reflected in an individual's verbal reactions to other persons as well as in other forms of behavior. A more legitimate reason for discarding the attribute is the fact that it is not a universal characteristic of the individual. The self is a conceptual construct, and a conceptual knowledge of the self implies a considerable amount of intellectual development. Presumably animals and infants have not attained such a stage of development, and consequently the possession of this attribute is limited to the human adult.

3. The third change was one of terminology. The term personality was substituted for that of the self, since the term

self contained too many subjective implications. Personality was regarded as a more appropriate term for an objective account of the individual.

Most of the early treatments of personality were similar to those of the self. The individual was discussed mainly under the categories of reactive tendencies, integrative unity, and growth and development, but those phenomena were described in more objective and behavioristic terms than formerly.

The recent treatment of the individual has also been influenced by the rise of the various applied psychologies, such as the medical, educational, and industrial. It is the reactive nature of the individual in which an applied psychology is primarily interested. Abnormalities of conduct are symptomatic of some abnormal condition within the individual, and medical psychology is interested in knowing what is wrong with these individuals and how these abnormal conditions can be remedied or prevented. Industrial psychology is interested in the types of motive that actuate human conduct, and in the evaluation of an individual's abilities and traits relative to success in the various vocations. It is the nature of the individual that is the focal point of their interest.

It was soon quite evident that the conventional descriptions of the individual offered little of value to any of these applied psychologies. The applied psychologists were thus forced to study the reactive nature of the individual in terms of various new categories that were significant for their purposes. Due to their influence the individual has come to be described and characterized in terms of such additional categories as aptitudes, abilities, traits, interests, attitudes, desires, motives, drives, etc. It is now generally recognized that the individual, like all concepts, is both a unitary and a highly complex object that can be adequately described only in terms of a great variety of attributive characteristics.

Terminology.—To sum up, the object or topic of discussion is the psychological characteristics of organisms, as distinct from those that are the subject matter of such sciences as

anatomy, physiology, and zoology. An organism can thus be referred to as a psychological object. Three terms have been used for this purpose. The "self" was the accepted term in times past. "Personality" is the term that is most widely used at present. We have preferred to use the term "individual" for several reasons that may be briefly mentioned. While it is recognized that questions of terminology are relatively unimportant, yet there are legitimate considerations governing the choice of appropriate terms.

1. There is need of a term to refer to the psychological characteristics of a particular organism. "An individual" can be used for this purpose.

2. There is also the need for a general class term that will denote the characteristics that are common to all psychological individuals. The term "the individual" serves this purpose.

The same word is often used to denote either a particular or general meaning. When the word is used alone, the qualifying articles of "a" and "the" are necessary to mediate the distinction of meaning. When the word is used as a part of a discourse, the qualifying articles are usually unnecessary as the distinction of meaning is mediated by the contextual situation of which the word is a part.

3. It is sometimes desirable to refer to a collection of particulars, and the word "individuals" can be used for this purpose.

4. Organisms differ materially in respect to their psychological characteristics and the term "individual differences" is generally used to denote this field of study.

Certain texts evince some disposition to limit the field of study to that of individual differences. Differences imply similarity, and any adequate knowledge of psychological individuals can be obtained only by comparing them in respect to their similarities as well as differences. Individual differences constitute but a part of the total field of study.

5. Each individual exhibits a distinctive group or pattern of characteristics in terms of which he can be identified and distinguished from his fellows. The term "individuality" can be used to denote such a group of characters.

6. Psychology now studies both human and animal organisms. For systematic purposes, we thus need a set of terms that will refer equally well to the psychological characteristics of animal as well as human organisms. The term individual and its derivatives can be used in this way.

The term self can be used for the first three purposes of denoting a particular, a general, or a group meaning. The term is inappropriate, however, from the standpoint of the last three requirements. It cannot be used to refer to the phenomena of individuality and individual differences. Both from its popular and psychological usage, the term has acquired a distinctly human connotation, and its application to animal organisms violates our sense of appropriateness.

Personality, in spite of its wide usage, has many disadvantages.

(1) It can be legitimately used as a general term to denote the common characteristics of the class of objects under consideration; it can also be used to denote what we have called individuality; but the word cannot be used to denote either meaning as the occasion requires without the use of awkward qualifying terms.

(2) Although "personality differences" is permissible, most writers seem to prefer individual differences to denote this field of study.

(3) Personality is an abstract and general term, and it cannot properly be used to denote a particular individual. "Person" is the concrete term, and we here encounter the most serious limitation of this set of terms. Person especially and to some extent its derivative personality have acquired such a distinctively human connotation that their application to animals does violence to their established and accepted usage. We occasionally hear the statement made that an animal has a personality, but the term is here used in the sense of individuality. No one has seriously proposed that animals be called persons.

(4) Personality has the further disadvantage that its meaning varies with the author. Of the many usages of the term, five distinctive meanings may be briefly noted. (*a*)

Some writers use the term to denote the general characteristics of all psychological individuals. It is used as a class concept to denote what we have called the individual in the generic sense of the term. (*b*) A few psychologists use the term to denote all those characteristics that are peculiar to the human adult as distinct from those possessed by animals and infants. This meaning is similar to that of the self. (*c*) Some psychologists have used the term to denote the social characteristics of the individual. This usage apparently grew out of the movement to describe the self in objective terms, *i.e.*, on the basis of the individual's relations to other persons. Personality is thus similar to the social self of the older terminology. (*d*) Some authors restrict the term to those patterns of characteristics that differentiate one individual from another. This usage of the term is synonymous with what we have called individuality. (*e*) Finally there is the usage of a considerable number of popular and pseudoscientific authors who are accustomed to write long and impressively about personality without defining the term. From the context one would judge that the term is used to refer to some unique, mysterious, intangible, and very important aspect of human nature. When used in this way, it is obvious that the concept of personality serves much the same purpose as did the long discarded concept of the psychological soul.

The dichotomy of act and individual.—Our texts usually devote a large amount of space to an account of such psychological activities as perception, memory, imagination, reasoning, feeling, emotion and will. This account is then followed by a description of the reactive nature of the individual under the rubric of the self or personality. The materials of the text are thus organized around two distinct categories—the psychological activities on the one hand, and the reactive nature of the individual on the other.

The comment is frequently encountered that these two divisions of our texts exhibit but little logical or organic relation to each other. The reader often gains the impression that psychology is concerned with the study of two relatively unrelated topics.

This apparent schism has been accentuated by the rise of the applied psychologies. The applied psychologists are primarily interested in the reactive nature of the individual, and they find little of value in the customary descriptions of the perceptual and memorial activities. The opinion is sometimes encountered that a new type of psychology is demanded—a psychology in which the reactive nature of the individual is the central principle of organization. Perhaps the present deluge of books on personality is a result of this demand.

This schismatic trend finds its strongest expression in the medical type of psychology. With the adoption of the psychological point of view, psychiatrists borrowed the concept of integrative unity, and attempted to explain their phenomena in terms of dissociation. This concept served their purpose fairly well in dealing with the various trance states, but it was soon evident that it was wholly inadequate to account for most of the major phenomena of psychiatry. As a consequence psychiatrists rightly asserted that academic psychology contained nothing of value for their purposes, and hence they were confronted with the necessity of developing their own system of psychological concepts. It was from this need, presumably, that the psycho-analytic psychologies arose. It is to be noted that these latter psychologies consist of a system of principles that are regarded as equally applicable to both normal and abnormal development, and many of their adherents are inclined to regard them as substitutes for—rather than supplementary to—the academic type of psychology. One system is termed Individual Psychology, and the implication of the title is more apparent if it is transposed into that of the Psychology of the Individual.

This schism between the psychology of activity and the psychology of the individual is both unfortunate and illogical. On the one hand many significant features of these activities can find their explanation only in terms of the organism, while on the other hand all of our knowledge of the reactive nature of the individual is derived from a study of its activity. Psychology is dealing with the behavior of an individual, and neither the individual nor his behavior can be adequately understood except in terms of the other.

Two propositions may be advanced. (1) The schism is due to the fact that our introductory texts have failed to give an account of all of the significant features of those activities with which psychology is concerned, the term "psychological activities" being here used to denote the concrete acts of every day life such as lecturing, playing golf, selling insurance, operating a lathe, etc. (2) On the positive side, an adequate explanation of all of these features will necessarily involve a reference to all of the attributive characteristics of the individual, and the psychology of activity and the psychology of the individual will represent two mutually related and supplementary bodies of knowledge. In support of these propositions we shall briefly sketch six points of view from which these activities can and should be studied, and indicate their explanatory reference to the individual.

1. *Analytical and general description.*—The customary mode of attack is to analyze these acts into such constituent components as perception, memory, imagination, feeling, emotion, and will, and then to describe each of these components in terms of their general characteristics. The general characteristics of perception are those that are common to all perceptive acts irrespective of the percipient individual. Obviously such a list of the general features of the perceptive process will involve no reference to the individual, for we are describing this process as *abstracted* from the individual. Neither can any such characteristic find its explanation in terms of the individual. Consequently a chapter on the individual will find no place in any text that confines itself exclusively to this mode of attack. This mode of treatment is the predominant characteristic of most of our introductory texts, and it is for this reason that the final chapters on the self and personality exhibit so little relation to the previous material.

2. *Efficiency of performance.*—In the process of development, these acts have become organized in reference to the attainment of certain results, and hence they can be studied from the standpoint of their efficiency in achieving such results. Efficiency has been found to be a function of four groups of conditions.

(*a*) Efficiency of performance is a function of various environmental circumstances in which the act is performed such as noise, illumination, temperature, humidity, oxygen content, etc.

(*b*) Efficiency is also a function of the motivation and mental attitude of the individual. The attitude of the worker toward his job and his employer is an important problem for industrial psychology. Educational psychology is concerned with the attitude of the child toward his teacher and the school situation. Some attempt has been made to study the influence of incentives and motives in the shop and school room, but the problem of experimental control is a serious obstacle. Consequently animals have been employed as subjects, and we have a very considerable number of experiments on this topic.

(*c*) Efficiency has been found to be a function of certain fluctuating mental and organic conditions. The temporary effects of such drugs as alcohol, nicotine, and caffeine have been systematically studied. The influence of fasting, loss of sleep, fatigue, emotional disturbances, etc., has also been subjected to experimental investigation.

(*d*) Finally efficiency of performance is a function of the constitutional nature of the individual. The term constitutional nature denotes all of the relatively persisting and enduring features of the organism, and they may be either innate or acquired in respect to origin. Let a group of individuals be tested for efficiency of performing a specified task under the following experimental conditions: (1) All individuals are tested under exactly the same set of objective circumstances. (2) All individuals are equated in respect to motivation and attitude toward the task. (3) The individuals are selected so as to eliminate the possible effect of the third group (*c*) of influences. Under these experimental conditions the obtained efficiency scores will exhibit a considerable amount of dispersion, and it is obvious that this distribution of scores cannot be explained in terms of the first three groups of conditions. Presumably it must find its explanation in terms of the constitutional differences of the

individuals, and this hypothesis can be validated by repeating the test. If the distribution of scores is an expression of the constitutional and hence enduring differences of the individual, it should be approximately reduplicated on a second test.

Efficiency of performance thus varies with these constitutional differences, and we are accustomed to denote this fact by saying that these individuals differ in ability.[1] Ability thus denotes that efficiency of performance which is a function of the constitutional nature of the individual and that can thus be approximately repeated in the future. Ability is both an explanatory and a predictive concept. Any adequate explanatory account of this topic of efficiency of performance will necessarily involve a reference to the reactive nature of the individual under the category of ability, and this concept of ability should be discussed in connection with the topic of efficiency as well as in the subsequent chapter on personality.

This topic of efficiency of performance has had a long experimental history. Much of the work has been carried on in a desultory fashion. An organized presentation of the experimental data can usually be found in the experimental texts. With several noteworthy exceptions, no reference to the topic is found in our introductory texts, though many of these do discuss the concept of ability under the rubric of personality. Presumably this topic has never been in good repute because of the stigma of the "practical."

The texts of Watson and Gates are two noteworthy exceptions. Watson[2] has a chapter entitled The Organism at Work, and Gates[3] presents the material under the title of Efficiency. Both give an excellent account of the experimental studies of the first (*a*) and third (*c*) groups of conditions previously listed. Gates also discusses the factor of motivation, and Watson refers to sex differences. However,

[1] H. A. Carr & F. A. Kingsbury. The concept of ability. Psychol. Rev., 1938, 45, pp. 354–376.

[2] J. B. Watson. *Psychology from the standpoint of a behaviorist.* (2nd ed.) Philadelphia: J. B. Lippincott Co., 1924, Chap. X.

[3] Gates. *Elementary psychology.* (Rev. ed.) New York: Macmillan Co., 1929, Chap. 18.

no mention is made of the relation of efficiency to the individual under the rubric of ability, and neither is this concept discussed in the chapters on personality.

3. *Manner of acting.*—Individuals may perform the same act, and yet the adverbial characteristics of this act may exhibit wide variations. The manner of walking or talking, for example, varies with circumstances and with the individual. Society has noted and named a great variety of these behavior characteristics because they are regarded as important factors in determining an individual's efficiency in meeting the various problems of life. Obviously such characteristics of behavior deserve psychological consideration. The occurrence of such a mode of behavior can be explained in one of three ways.

(*a*) A salesman may give essentially the same sales talk to a number of prospective customers, but he will intentionally vary his manner of presentation in accordance with what he knows about the reactive nature of each individual. These modes of behavior may sometimes be regarded as intelligent adjustments to circumstances, and in so far as this is true, their nature will vary with those circumstances.

(*b*) These adverbial characteristics are sometimes an expression of some of the temporary and fluctuating conditions of the subject. The petulance and peevishness of the sick room is an illustration. Children often act in socially disapproved ways when they are not feeling well, and wise parents generally ignore these deviations from accustomed behavior because they recognize their cause and know that they will soon disappear.

(*c*) Finally these adverbial characteristics may be an expression of the constitutional nature of the individual. The individual acts cautiously, courteously, sullenly, or aggressively simply because it is his nature to do so. In this case, we can legitimately describe the reactive nature of an individual in terms of these characteristics, and he is defined as a cautious, a courteous, a sullen, or an aggressive person. These definitions of the reactive nature of an individual on the basis of his constitutional manner of acting

are called traits,[4] and one author[5] has recently compiled a list of about 18,000 trait names. A trait is thus an explanatory and descriptive concept in that it defines an individual in terms of those ways of behaving that find their explanation in terms of his constitutional nature, and it is also a useful concept for purposes of prediction.

Many of these adverbial characteristics of behavior are of sufficient importance to merit psychological treatment, and any adequate explanatory account of these characteristics will involve a reference to the reactive nature of the individual under the category of traits. The concept of traits should thus be discussed in connection with this topic of behavior. Our introductory texts, however, are accustomed to discuss this concept in the chapter on personality without any reference to the concrete behavior situations from which it originated.

4. *Selectivity of response.*—As previously stated, the acts with which psychology is concerned have in the course of development become organized in reference to the attainment of certain results, which are usually termed the goal or objective of the act. It is these objectives that are socially significant, and hence society is accustomed to label acts, either explicitly or implicitly, in terms of their objectives. We thus speak of such acts as going home to lunch, preparing a lecture, waiting on customers, taking a walk through the park, studying for the ministry, etc.

A complex organism like man has a great variety of objectives in reference to which he acts from time to time, and these objectives are perceptual or ideational, and concrete or highly abstract in character. A human individual thus possesses an extensive repertoire of acts, but he can perform but one at a time, and the nature of the act varies with circumstances. This fact has usually been referred to as the phenomenon of "selectivity of response." Obviously no account of these psychological activities can be considered

[4] H. A. Carr & F. A. Kingsbury. The concept of traits. Psychol. Rev., 1938, 45, pp. 497–524.

[5] G. W. Allport. Trait-names. *Psychol. Monogr.*, 1936, 47, pp. 1–171.

complete that does not include a formulation of the general principles involved in the explanation of why organisms act as they do in particular circumstances.

It is obvious that this phenomenon finds its explanation in part in terms of the nature of the organism, and society has developed a set of conceptual terms to denote the variety of organic conditions of which the character of our acts is in part an expression. Psychology has borrowed many of these popular terms and has added some of its own. What an individual does is thus regarded as an expression of his ideals, interests, attitudes, desires, wants, needs, appetites, sets, tensions, drives, dispositions, etc. Some of these terms denote legitimate distinctions, but there is much overlapping of meaning, and the customary accounts of this phase of psychology suffer from the lack of a commonly accepted and distinctive set of terms.

The above terms are class concepts, and it is necessary to identify their particulars in terms of their objectives, as an interest in art, an attitude toward religion, a desire for wealth, and a need for exercise. All of these particular terms have one attribute in common in virtue of which they can be grouped into a single class. This common characteristic is their objective reference, and hence they may be termed "directional dispositions,"[6] viz., those organic conditions which dispose the organism to act in reference to some objective.

In respect to their origin these directional dispositions are explanatory concepts, and any adequate explanation of this phenomenon of selectivity of response must involve a reference to the individual in terms of these concepts.

Inasmuch as these concepts denote certain conditions within the individual that are reflected in its behavior, they can also be used to describe the reactive nature of that individual. It is this descriptive usage of the terms that is encountered in every day life, where we are all accustomed to characterize our acquaintances in terms of some of their

[6] F. A. Kingsbury & H. A. Carr. The concept of directional dispositions. Psychol. Rev., 1939, 46, pp. 199–225.

predominant and distinctive dispositions. An individual is thus characterized as a man who is intensely interested in art, who is antagonistic toward religion, or who is actuated by a desire for great wealth. It is commonly assumed that human individuals have something in common in respect to their objectives, and it is these characteristics of "human nature" with which the social sciences are much concerned. Some sociologists are accustomed to describe human nature in terms of four classes of desire—the desire for security, the desire for novel experiences, the desire for response, and the desire for prestige or social recognition, and it must be admitted that this formulation has some value for their purposes.

The concept of directional dispositions, like those of abilities and traits, has a dual function. As noted in the previous sections, most texts employ the concepts of abilities and traits as descriptive devices in the chapters on personality without any reference to their significance in explaining certain features of behavior. These texts adopt a somewhat different attitude toward the concept of directional dispositions, for they have been considerably influenced by the motivational and dynamic trend in psychology, and hence devote a considerable amount of space to the explanation of certain features of behavior in terms of such concepts as motives, appetites and drives. No such explanatory use is made of the concepts of interests, attitudes and desires, though the later chapters on personality often contain some vague reference to interests and attitudes as descriptive terms.

5. *Genesis.*—Every psychological act has had a long genetic history, and no account of these acts can be complete without some reference to their development. All of our text books have recognized this fact. Perception, for example, involves an interpretation of sense data in terms of previous experiences, and obviously this reference to the past is the distinctive characteristic of memory.

Our early psychologists saw that this functional dependence of the present upon the past is no isolated or restricted phenomenon, but that the genetic roots of some features of every act can be traced back through the entire life history of

the organism. They thus formulated the doctrine that all of the activities of an organism throughout life become organized into a functional unit, and that each succeeding act is to some extent a function of that organization. It is this organization that we have termed "integrative unity," and the formulation of this concept must be regarded as a noteworthy achievement.

It is universally admitted that this fact of integration must find its explanation in terms of the organism. We all assume that each act modifies to some extent the reactive nature of the organism and thus influences its subsequent activity. These persistent effects have been variously named, *e.g.*, engrams, traces, and pathways, and there has been much speculation concerning their nature and locus. While the existence of these effects is a necessary inference, it is well to recognize in some explicit fashion that we know nothing of their nature or locus except that the cerebral tissues are involved. In fact we do not even know that they are *exclusively* neural in character.

The reactive nature of an organism can thus be defined in terms of this phenomenon of integrative unity. As previously stated, it is these definitions of the reactive nature of an organism as revealed in various features of its psychological conduct that constitutes the attributes of the individual. What we call the self, the personality, or the individual must thus be regarded as a *unitary* object.

The suggestion is sometimes encountered that since personality is unitary in character, it is therefore improper to define it in terms of a multitude of traits, abilities, and dispositions. This doctrine is apparently based upon the erroneous assumption that unity and complexity are mutually exclusive terms. Unity and complexity, however, are relative terms, and one implies the other. There can be no unity without complexity, and vice versa. Like all concepts, the individual is both a unitary and a complex object.

The formulation of this concept of integrative unity, however, does not constitute an adequate account of psychological development, for we still need to know something about (1)

the principles and mechanisms involved in this integrative process, and (2) the various factors that influence rate of development. We here encounter the very extensive series of experiments on learning. These experiments are obviously concerned with the study of this integrative process for certain selected activities, for a short interval of time, and under laboratory conditions.

First, these experiments are concerned with the discovery and formulation of the general principles and mechanisms involved in such analytical features of the learning process as problem solving, selection and elimination, retention, retroactive inhibition, transfer, and generalization. In spite of the extensive amount of work that has been done, it is generally admitted that our present formulations of the principles and mechanisms of learning are far from complete and satisfactory. From the standpoint of systematic psychology, it is also well to recognize the fact that these laboratory studies are significant only in so far as they lead to the formulation of a set of principles that are adequate to an understanding of the nature of the integrative process as it is continually occurring in our every day activities. We may also note that principles derived from laboratory studies must frequently be subjected to considerable qualification and modification when applied to the complex conditions of life.

Second, the influence upon rate of learning of various factors such as kind and length of material, motivation, method of learning, distribution of practice, and the organic effects of alcohol, nicotine, and loss of sleep, has been extensively studied. Rate of learning is also a function of the constitutional nature of the individual, and no explanatory account of rate of learning is complete without a reference to the individual under the concept of learning ability. This latter aspect of the problem has been relatively neglected. While the existence of individual differences in the ability to learn has been experimentally demonstrated, yet little is known as to the extent to which these differences are general in character and the extent to which they vary with the nature of the problem and the material.

The I. Q. scores are generally regarded as measures of those differences in *rate of integrative development* that are functions of certain innate differences between individuals. The I. Q. and learning ability as derived from laboratory studies are thus related concepts. As developed in a previous article,[7] the I. Q. scores are derived under such conditions that they may be well regarded as the more typical and representative measures of rate of integrative development as incident to our everyday activities. For this reason, the I. Q. is frequently defined as a *substitute* measure of learning, or as a measure of that rate of mental growth that is a function of innate conditions.

Rate of integrative development thus cannot be completely explained except in terms of constitutional conditions, and the innate conditions of mental development are usually apprehended under the concept of the I. Q. Like other characteristics of the individual, the I. Q. is primarily an explanatory concept, and it consequently may be used to describe the reactive nature of the human organism as it is revealed in rate of development.

Many of these attributive characteristics of the individual are developmental products. It has long been recognized that the character of an individual's abilities, traits, interests, and desires has to a large extent been learned. Does the acquisition of these characteristics involve any principles of learning that are different from those involved in the acquisition of knowledge and acts of skill, or are both to be explained in terms of the same principles and mechanisms? Every one assumes that the acquisition and fixation of new modes of behavior are to be explained in terms of a modification of the reactive nature of the organism. We have also noted that all of these attributive characteristics of the individual denote those conditions in the organism that are causally reflected in its behavior. It would thus seem that both acts and attributive characteristics should develop together as products of the same integrative process. This hypothesis has recently

[7] H. A. Carr & F. A. Kingsbury. The concept of ability. Psychol. Rev., 1938, 45, pp. 354-376.

been subjected to an experimental test by Thorndike,[8] and he concluded that the development of such characteristics as wants, interests, and attitudes is to be explained in terms of the same principles of learning as is the acquisition of knowledge and acts of skill.

6. *Self consciousness as a genetic factor.*—As previously stated, self consciousness was once regarded as one of the important characteristics of the individual. This attribute has been discarded as an essential attribute of the individual, however, on the grounds that its existence is limited to the human adult, and hence it is not an attribute of all psychological individuals.

Our sociologists and social psychologists have long insisted that this fact of self consciousness is not only a developmental product, but that it is a product that exerts a very pervasive and profound influence upon the character of later development. If self consciousness does exert such an effect, it is obvious that any adequate genetic study must involve some reference to the mechanisms involved in this cause and effect relation. To understand this relation, we may note that the term "self consciousness" means a knowledge of one's self and that this knowledge influences subsequent development because it exerts some effect upon conduct. For purpose of illustration, we shall cite a few conspicuous examples of the influence of this cognitive appraisal of one's self in reference to concrete behavior situations.

A. The first example is that of "frustration." This term is used to denote those failures that are sufficient in magnitude, and which occur under such conditions, that the fact of failure constitutes a stimulating condition that arouses some distinctive mode of reaction in reference to it. As a concrete illustration, we shall take the case of a first year college student who fails a sufficient number of courses to jeopardize his continuance in college, and who really wants to complete his college career because of desire, personal pride, family and social pressure, etc. Under these conditions, the fact of failure constitutes a personal problem that demands some sort of a solution.

[8] E. L. Thorndike. *The psychology of wants, interests, and attitudes.* New York: D. Appleton-Century Co., 1934.

There are several ways of reacting to such a provocative situation, but we shall limit the discussion to the various explanations that may be adopted. On the one hand, the student may assume that his failure is due to defective ability, poor high school preparation, financial worries, emotional disturbances, poor habits of study, lack of will power, too much outside work, or too many college distractions. On the other hand, he may decide that his failure is due to some bias or hostility on the part of his instructors because of his race, color, nationality, social status, or some aggravating set of personality characteristics. In explaining this alleged bias, it is possible for an extremely suspicious individual to draw somewhat heavily upon his imagination, and thus develop some incipient symptoms of a persecution complex.

Each of these explanations involves a reference to the self, and each has certain behavior implications. The explanatory possibilities will vary to some extent with the behavior situation. The explanation adopted and the resultant mode of reaction will vary with the individual. The explanation adopted may be either correct or incorrect, and it may be even somewhat irrational in character. Psychology, however, is not concerned with the truth of the adopted explanation, for the individual will act on the basis of this explanation irrespective of its correctness or rationality.

B. Outstanding success is a second illustration. It is generally recognized that outstanding success in any walk of life is due to such factors as unusual ability, family and social connections, and certain fortuitous or chance circumstances. Successful people are prone to minimize or ignore the influence of the last two factors, and to assume full credit for their success. They are also prone to adopt a generalizing attitude toward this type of explanation, and to assume in some tacit fashion that they would have been unusually successful in any line of activity. As an illustration, we may cite the conventional example of the self made millionaire who can pass final judgment on any question, and give confident advice to a class of graduates on how to succeed in life. It is well to recognize, however, that pertinent examples could have been selected equally well from any walk of life. Perhaps it is

unusual failure that exerts the more pervasive and detrimental effect upon subsequent development, but outstanding success is probably responsible for some of the more irritating and obnoxious forms of personality characteristics.

The effect of success and failure upon conduct and subsequent development is not limited to the conspicuous cases cited in the above illustrations. Success and failure always exert some effect upon conduct, and this fact is recognized in the so-called "law of effect" in learning, viz., that an individual's mode of behavior in reference to the problematical situation is modified from trial to trial on the basis of his success and failure in the preceding trials.

In the conspicuous cases cited, success and failure involve an explanatory self-reference, and they exert a very general, wide-spread and pervasive effect upon subsequent development. In the usual learning situation, on the contrary, no explanatory self-reference is present, and success and failure exert a very limited effect in that they modify the individual's behavior in reference to a particular situation.

Psychologists have frequently noted that this generality of effect occurs under some conditions, but not under others, and they have had some difficulty in accounting for this observed fact. We wish to suggest that it is these explanations in terms of the self that are at the root of this generalizing function. Let us assume that our college student firmly believes that his failure is due to racial prejudice. Naturally the resultant sense of injustice may operate to influence the character of his reactions to a very wide class of social situations. Again let us suppose that he explains his failure in terms of a deficiency of ability. This factor of defective ability is also quite general in respect to its behavior implications. In fact all characteristics of the self in terms of which success or failure may be explained necessarily possess some degree of permanence and general significance, for otherwise they would not be regarded as attributes of the self.

C. A final illustration is the sense of guilt. There is the case of some serious indiscretion or shocking experience in early life that so violates the accepted canons of society and the individual's own sense of values that he regards himself

as a guilty person and is subject to fits of remorse. Two suggestions have been made to account for the detrimental effects of such experiences. One view assumes that the individual forgets these experiences in order to escape the sense of guilt and the feeling of remorse, and that these forgotten experiences then disturb future conduct by way of some indirect and subterranean channels. The second conception assumes that the sense of guilt and the feeling of remorse directly affect conduct, and that their disturbing effect upon development is due to the inability to forget.

We may also cite the secret vices and practices of the adolescent stage of development. Again the individual is prone to regard himself as a guilty person and be subject to occasional fits of shame and remorse. This situation may be further complicated by the haunting fear that his transgressions may be detected or suspected by his associates, and the poignancy and persistence of such fears is well attested by the confessions of escaped criminals. Such attitudes will necessarily affect the individual's behavior to his immediate social environment. It has even been suggested that the withdrawal symptom of dementia præcox may find its origin in such situations.

These three illustrations suffice to indicate that self consciousness, when properly interpreted, is an effective factor in development, and that the mechanisms involved are quite complex in character. We now wish to note that, for purposes of exposition, we chose illustrations in which this factor is likely to exert some rather conspicuous and even abnormal effect upon subsequent development. This choice, however, has the disadvantage of conveying the impression that the operation of this factor is limited to an extremely small number of behavior situations and that it accounts only for the deviate types of development. Hence we wish to emphasize the fact that this factor operates in a great variety of behavior situations, and exerts some effect upon normal development. As a concrete example, we may cite the critical appraisal of one's self in reference to the choice of a mate or a vocation, for it is evident that the character of one's subsequent development may be materially influenced

by that choice. Any adequate account of the influence of this genetic factor of self consciousness must contain a systematic statement of the various mechanisms involved, and depict its effect upon both normal and abnormal development.

We previously suggested that the psychoanalytic type of psychology found its origin in the inadequacies of academic psychology. We are not at all concerned with these psychoanalytic psychologies from the standpoint of the etiology and treatment of the psychoses. We merely wish to note that these systems purport to be a psychology of the genetic and developmental sort. As is well known, most psychologists are skeptical of the validity of some of the suggested mechanisms of development, and it is generally believed that no one of these systems gives an adequate account of all of the significant features of human development. In spite of this critical attitude, many psychologists believe that these systems do contain much that is of value to a systematic psychology, and as a consequence we find that many of the current texts devote some space to a discussion of such concepts as repression, compensation, rationalization, defense mechanisms, etc. Many of these discussions, however, do not exhibit any very obvious logical relation to the rest of the text.

We now wish to suggest the hypothesis that the central topic with which these psychoanalytic psychologies are in the main concerned is this neglected factor of self consciousness and the various mechanisms by which this type of knowledge does exert a distinctive effect upon subsequent conduct. If the truth of this hypothesis is granted, it is evident that any system of psychology that contains an adequate account of the genetic influence of this factor of self consciousness will also adequately serve the needs of this medical type of psychology.

Summary.—We have raised the question of the function of the concept of the individual in a systematic psychology. We have noted that the materials of our introductory texts are organized around the two categories of psychological behavior and the nature of the individual, and that these two divisions do not exhibit any very obvious relation to each other.

It has been suggested that this logical schism is primarily due to an inadequate and incomplete account of these psycho-

logical activities, and that a complete explanatory account will include all that we wish to know about the reactive nature of the individual. Various features of these acts find their explanation in part in terms of conditions within the organism. Since their organic nature is unknown, these conditions can be denoted only in conceptual terms that refer to those features of behavior from which their existence is inferred. These terms are primarily explanatory concepts, and they can therefore be used to describe the reactive nature of the organism as it is causally reflected in its behavior.

On the basis of the functional interrelationships of an organism's acts we assume that all of these organic conditions are integrated into some sort of a functional unit. The concept of the individual is used to denote the entirety of these conditions in respect to their psychological significance, while the concepts of abilities, traits, and directional dispositions are used to specify these conditions as they are revealed in certain particular features of behavior. The individual is thus a unit whose nature can be further specified in terms of such attributive characteristics as abilities, traits, interests, attitudes, etc.

It is thus possible for a text that purports to be systematic to organize all its materials around the single category of psychological activity, for any adequate explanation of all of the significant features of these acts will necessarily involve a complete account of the reactive nature of the individual. While a final chapter on the individual is unnecessary, yet its inclusion is advisable for it will serve to bring together in an organized fashion all of the previous references to the individual. Additional chapters concerning some of these attributive characteristics are also legitimate in an introductory text for they will serve to orient the beginning student in reference to some of these special fields of study. So long as all of these concepts are encountered in the explanatory account of the various features of behavior, the relation of these final chapters to that which has gone before will be logically apparent.

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ENVIRONMENTAL DETERMINANTS IN PSYCHOLOGICAL THEORIES

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A. Introduction

Numerous attempts have been made to bring about discussions in which the basic concepts of different psychological theories can be brought into relation with each other. But such attempts have usually failed to yield fruitful results, and they have failed largely because the basic terms in which the data are organized by the different theories are not fully analyzed and discussion is often based on concepts that are not directly comparable. In order to bring together the different theoretical approaches to psychological problems and to discuss them, we must recognize that it is often possible, especially in sciences dealing with the organism, to organize data in several different ways, at least in a first approximation. The way in which data are organized and the method by which the identity of focal unities or focal variables is determined are significant characteristics of a theory. Until we understand clearly what a given theory considers essential for the description of phenomena and in what terms it describes a given process in order to connect it with what has preceded and to predict its future, we cannot try to bring that theory into relation with others.

The purpose of this paper is to consider some general problems of determination and of the derivation of subordinate systems. The distinction between proximal and distal determinants is treated in detail and the attempt is made to analyze several psychological theories from the point of view of this distinction.

B. General Remarks About Determination of Focal Variables

I. *Relevant determination.*—In order to discuss the determination of focal variables we can do no better than to begin with a quotation from Holt:

> . . . it is inaccurate to say that a river flows toward the sea . . . while it is fairly accurate to describe it as always flowing towards the next lower level of the earth's surface, and this is a law describing flow as a constant function of the earth's crust and the position of the earth's center. The test is, of course, whether this or that could be removed *without changing* the river's course. . . . So in behavior, the flock of birds is not, with any accuracy, flying over the green field; it is, more essentially, flying southward; . . . the sole question which we need ever ask is, 'What is it doing?' (11, p. 166).

Or again:

> . . . the man is walking past my window; no, I am wrong, it is not past my window that he is walking; it is *to* the theater; . . . the functional view . . . admonishes us to keep the man whole (if it is *behavior* that we are studying) and to study his movements until we have discovered *exactly what* he is doing, that is, until we have found that object, situation, process . . . of which his behavior is a *constant function* (11, p. 161 f.).

Koffka, in explaining the meaning of relevant and irrelevant description, uses almost the same example:

> . . . a ball runs down an incline and finally falls into a hole. Now there may be water in the hole or not, and therefore I can say the ball falls into a hole with water or without water. But this difference does not affect the motions of the ball until it has reached that position in space where the water begins in the one case and not in the other. For the rest of the motion the presence or absence of water is wholly irrelevant; similarly, the statement that the rat does not run towards food when the experimenter has just removed it, is quite irrelevant to the run of the rat

until it is near enough to notice the absence of food (16, p. 37).

Of course the inquiry, "What are the objects doing?" is not so simple as Holt makes it out. Each theory uses a different set of concepts as the only valid and legitimate one. But we learn from these quotations first, that there exist a great number—as a matter of fact an infinite number—of possibilities of determining an event. We can determine the motion of the water in a river as towards the sea or towards the next lower level of the earth's surface; we can describe the motion of a flock of birds as one "over the green field," or as a "southward" one. Besides the determinants mentioned in the quotations, a great number of other determinants are possible.

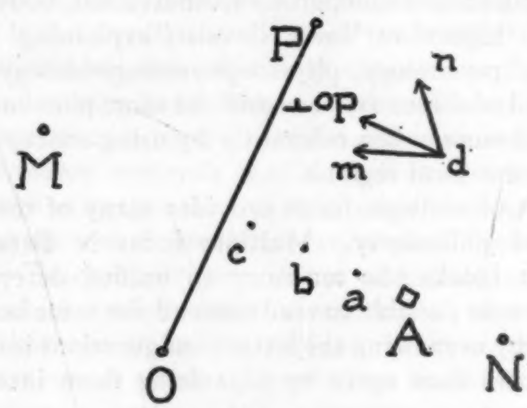

FIG. 1

Secondly we learn that only a few determinants are relevant; *i.e.*, there are only a few determinants that can be used to describe the events in a simple way. The test for this relevancy consists in finding constant functions. We may add another example which demonstrates the same facts in a more abstract way. Let us assume that one can observe that particle *A* in Fig. 1 moves through points *a*, *b*, *c*. We are free to determine this motion in many different ways. We can say *A* is moving toward *M*, or away from *N*, or that it is moving toward the line *OP* in a direction perpendicular to it.

Which determination is relevant? That is to say, what is *A* 'really' doing? We find the answer if we remove *A* to point *d*. If *A* then moves in direction *m* it is moving toward *M*. If it moves in direction *n* it is moving away from *N*, and so on.[1]

II. *Rival determinants.*—However, the task of determining relevantly is not always so simple. It is often made difficult by the possibility of rival determinants. Usually only one determinant will be successful; that is to say, will make a simple description of events possible and will yield constant coordinations for different situations. However, it often happens that determinants which belong to different regions seem to yield constant functions. Wherever we find two or more contending theories there exists such a situation. Examples are: description in terms of the whole or in terms of the parts (gas *vs.* molecule, group *vs.* individual, body *vs.* cell); in terms of 'higher' or 'lower' levels ('explaining' and 'understanding' psychology, physics-physiology-biology-psychology). In all of these cases one and the same phenomenon can be described more or less relevantly by using concepts belonging to different focal regions.

The fact of multiple focus provides many of the classical problems of philosophy. Multiple focus is distasteful to thinking; it blocks the tendency to unified determination. It is as if it were possible to read one and the same book in two ways; once by organizing the letter configurations into English sentences, and then again by organizing them into German sentences.

III. *Types of derivation of subordinate systems of determinants.*—In order to escape the dilemma of rival determinants different devices are used. Thus, many theories say: "Yes, it is true that one can find more or less constant functions if one chooses terms of region *B*. However, using these determinants, one will never attain a complete description of the cases in question. The seemingly constant functions one finds in this way are the product of a combination of functions which can only be determined adequately if one

[1] *Cf.*, in regard to methods for finding relevant determinations, the important studies of Heinrich Klüver (14, 15).

applies terms of region *A*." The derivation of subordinate systems of functions becomes, therefore, a main problem for many theories.

This derivation follows certain patterns. A typical kind of derivation is, for instance, derivation by selection. Let us assume that a given manifold of processes or objects can be determined more or less relevantly in terms of two different regions, *A* and *B*. This is a case of multiple focus. Let us now assume that one theory, in order to reduce this multiple focus to single focus, declares *A* to be the focus, the independent system, and *B* to be the derived system.

The theory contends, then, that originally there were an infinite number of processes or objects, and they were all relevantly determinable in system *A*. The fact that we find a few processes which show constancy when determined in system *B* is due to chance. If there is an infinite number of items available we can arbitrarily choose any determinants and we shall always find a certain number of items which show constant functions in regard to those determinants. Furthermore, the theory contends that there is a selection going on which works in such a way that in the end only items conforming to system *B* are left over, and all items not conforming to *B* are eradicated. Since all items conform to *A*, the items which are left over conform to both *A* and *B*. It is assumed that the selection itself can also be explained in terms of system *A* alone. Thus it can be shown that only determination in system *A* is inherently relevant. Determination in *B* is not independently relevant; it is an outgrowth of determination in *A*.

Darwin's theory of selection uses this method in establishing the system of teleological determination as a dependent system. An infinite number of possibilities is realized, all conforming to system *A*, which is in this case the system of causal explanation. Some of them possess characteristics which 'make sense' in system *B* also; that is to say, they can be determined as having some use for the organism. Natural selection then works in such a way that only these possibilities are left over.

Other cases of derivation by selection are offered by the theories which explain the teleological properties of perception or thinking, namely the fact that in some way they show a correspondence to the 'real' world, in terms of experience. The problem is very similar: Double determination, that is to say, determination in causal and in teleological terms, has to be reduced to single determination in causal terms. An infinite number of possible bonds is realized, all conforming to causal system *A*. Some of them also fit the teleological system *B*, and a law of effect, for instance, takes care of the selection. Theories of association, of conditioned reflex, positivistic and pragmatistic theories of the derivation of logical forms, all make use of this type of derivation.

Often the defenders of system *B* use counter-arguments, which again follow typical patterns. They can say, first, that the assumption of an originally infinite number of possibilities is untenable. In observation we find only a small number of items $A\bar{B}$ (conforming to *A* but not to *B*); most of the items are *AB*. It cannot be due to chance, therefore, that *B*'s appear.

Or they can say that the *B*'s are of such great complexity that the probability of *B*'s coming into existence among the *A*'s through chance is infinitely small.

Or they can say that there exist *B*'s in regions where the process by which *B*'s were selected from the infinite number of *A*'s never occurred.

To this last argument the defenders of *A* can retort by using the device of extrapolation. They assume that selection worked so often on items conforming to system *A* that these items, in time, got used to conforming to *B* and so exhibit *B*-characteristics even in regions where the selection was not directly active.

IV. *Tendencies affecting the selection of determinants.*—One might assume that the question as to which region shall be used as the primary system of determination is always answered on the basis of empirical findings. However, this assumption does not always seem to hold. General *a priori* tendencies are often responsible for the choice of a certain

region as a focus. A few of these tendencies may be enumerated.

a. A tendency to use regions close to the observer as the focal region. This tendency is the result of a reluctance to make theories and of an inclination to premature measurement. Surface processes or surface characteristics are taken as relevant determinants and correlations are computed. Both the psychology of personality and of social functions provide many examples. This tendency is supported by positivism. The important principle which states that every concept should be anchored in observations or 'operations,' is often misunderstood. It is thought that one should take the directly observed entities themselves as focus and that one should be satisfied with a language of data. Cruder forms of behaviorism thus find an ally in a misunderstood positivism.[2]

b. A tendency to use the same focus for different regions of a science. One does not want the science to be broken up into regions with different determinants. If that is done there will always be attempts to find constancies of a higher order and to put the focus into a deeper layer so as to combine the different regions.

c. A tendency to use proximal determinants and to avoid distant determinants. No change should be determined by something which is distant in time or space. More will be said about this point later.

V. *The psychological fallacies.*—In determining the focus of a psychological theory one has always to keep in mind the possibility of a disguised focus. The relevant variables may be assumed to be in region *A;* however, since there often exists a confusion between regions, or an unwarranted assumption of equality of regions, the relevant variables are really described in terms of another region *B.* By means of a disguised focus sham solutions of many problems are possible. This fact has often been noticed, and it has been discussed in a more general way under the name of 'fallacy' or 'error.'

[2] *Cf.* Koffka's concepts of achievement and performance (16, p. 530); Lewin's confrontation of historic-geographic and systematic concepts, phenotypic and genotypic language (18).

Some of these confusions (*cf.* Table I) are:

a. James' 'psychologist's fallacy': The 'mental state' is determined in terms of the coordinated object. 'Equal to' is substituted for 'referring to.'

> The *great* snare of the psychologist is the *confusion of his own standpoint with that of the mental fact* about which he is making his report. I shall hereafter call this the 'psychologist's fallacy' *par excellence*. . . . Now when it is a *cognitive* state (percept, thought, concept, etc.), he ordinarily has no other way of naming it than as the thought, percept, etc., *of that object*. He himself, meanwhile, knowing the self-same object in *his* way, gets easily led to suppose that the thought, which is *of* it, knows it in the same way in which he knows it, although this is often very far from being the case (13, Vol. I, p. 196).

TABLE I

SOME OF THE PSYCHOLOGICAL 'FALLACIES'

The arrows start in the region, the terms of which are supposedly used; they end in the region, the terms of which are actually used.

	images	sensations	stimuli	objects
James' fallacy	———	———	———	——→
Stimulus error (Titchener)		———	———	——→
Constancy hypothesis		———	——→	
Experience error			———	——→

b. Titchener's stimulus error: The psychological 'elements' (sensations, etc.) are described in terms of the physical environment. "The psychologist commits the stimulus-error when he lapses from the psychological point of view into some other, like the physical" (Boring, 2, p. 410).

c. Köhler's constancy hypothesis: 'Sensations' are hypothesized and described in terms of the stimuli (17, p. 96).

d. Köhler's experience error: Stimuli are described in terms of the experiential or distant object.

> In psychology much has been said about the stimulus-error which consists in our confusing our knowledge about the physical conditions of sensory experience with experience as such. But another mistake, which I propose to call the *experience-error*, is not less unfortunate. It occurs when we unintentionally attribute certain properties of

sensory experience to the actual constellation of stimuli, properties which are so very common that we tend to apply them to whatever we are thinking about. This is the case primarily, wherever we have not yet learned to see the *problem* contained in those common properties of experience. No wonder, then, that neurologists and some psychologists still talk about 'the retinal stimuli' corresponding to an object, as though there were something like detached functional units on the retina (17, p. 176).

Because the distant object is a thing by itself, the assumption is tacitly made that the retinal image corresponding to it is also (16, p. 97).

C. Distal and Proximal Determinants in Several Psychological Theories

A consideration of relevant description and its relation to a manifold of regions is so important for psychology because, as the discussion of the fallacies has shown, there are so many regions involved in each psychological process. And not only are there many regions involved, but determination in terms of each region seems to give sense; that is to say, leads to relatively constant functions. Therefore the multiplicity of psychological theories, each of which places the focal concepts in a different region.

In the following an attempt is made to describe a few psychological theories in regard to the place of focus. We shall, for the most part, consider only the outer field, that is to say, environmental determinants, and we shall only occasionally refer to internal, inner-organismic determination.

Of prime importance for all theories is the question whether distal or proximal data are used as the focus in the determination. One can treat perception and action either in terms of the distant object (perception functions in such a way that the distant object is 'attained'; the organism moves towards the food, etc.); or one can treat it in terms of proximal influences and effects, that is to say, in terms of processes close to the skin, stimuli, muscle contractions or movements of the limbs.[3]

[3] *Cf.* Koffka's use of the terms proximal and distant stimuli (16, p. 80).

I. *Theories in terms of proximal influences and effects.*—These theories fall into two groups. One group stresses perception; to it belong the older theories of perception which emphasized the stimulus-oriented sensations. The other group treats the psychological processes more from the point of view of action and motor phenomena; to it belong the stimulus-response theories.[4]

It is easy to see that these theories get their vitality from the general tendency to use proximal determinants and not from observation; observation favors distal determinants much more. The exponents of these theories want to relate psychological processes to the actual concrete influences which organism and environment exert on each other.

The most important arguments against these theories can be reduced to a single point: Observation shows that often distal determination is possible where proximal determination is impossible. Von Kries, Becher, Ehrenfels, and the Gestalt theory used this argument against the older theories of perception; different teleological systems (McDougall, etc.) used this argument against stimulus-response theories.

Indeed, the most important problem for all theories using proximal determinants is to show that it is possible to establish that system of determination as the independent one, and further that it is possible to derive from that system the existence of relevant distal determinants, which are found in observation, and to treat them as only apparently relevant determinants. The device which is almost exclusively used for this derivation is selection. There is the infinite number of possibilities of bonds of association or conditioned reflexes between any stimuli and any responses. Contact with the environment establishes or strengthens only a limited, selected number of these bonds. Selection works in such a way that distal determination, that is to say, correspondence to the objects of the environment, is brought about.

However, very often the derivation of distal from proximal determination is effected by the surreptitious substitution of

[4] That these two groups of theories belong together has been shown, for instance, by Köhler (17).

distal for proximal terms.[5] The following cases are examples of disguised focus. We quote here from one study, where distal determinants are quite openly introduced into a 'stimulus-response' theory.

> Suppose that we are studying the behavior of such an organism as a rat in pressing a lever. The number of distinguishable acts on the part of the rat that will give the required movement of the lever is indefinite and very large. Except for certain rare cases they constitute a class, which is sufficiently well-defined by the phrase 'pressing the lever.' Now it may be shown that under various circumstances the rate of responding is significant—that is to say, it maintains itself or changes in lawful ways (Skinner, **19**, p. 44).

We note that the author starts out to study "the behavior of such an organism as a rat in pressing a lever," that is to say, a phenomenon which is from the beginning defined by distal determinants. It is conceded that it is impossible to find coördinated proximal events: "The number of distinguishable acts on the part of the rat that will give the required movement of the lever is indefinite and very large." Therefore, instead of proximal events, there is substituted a "class which is sufficiently well defined by the phrase 'pressing a lever,'" a phrase which of course denotes distal determination in its purest form. And, placing the focus in the distant environment, one can find that "the rate of responding is significant—that is to say, it maintains itself or changes in lawful ways," namely, distal determination yields constant functions.

That distal determination is forced upon us by the observed facts is also stressed in the following quotation:

> The uniformity of the change in rate excludes any supposition that we are dealing with a group of separate reflexes and forces the conclusion that 'pressing the lever' behaves experimentally as a unitary thing (**19**, p. 45).

We should like to add one more quotation from the same

[5] *Cf.* especially the keen analysis of stimulus-response theories by Arthur F. Bentley (**1**).

paper, in which the principle of finding the relevant determination is clearly expressed:

> It is then possible to test the irrelevance of a non-defining property by showing that two responses, one of which possesses the property, the other not, contribute equally well to a total number [of elicitations of a reflex] (19, p. 44).

Not always is the change in focus as patent as in this paper. It is much more difficult, for instance, to find in Hull's theory of family-hierarchy the place where distal determinants are substituted for proximal determinants. The problem that this theory attempts to solve comes from the fact that 'habits' which are determined distally, by the achievement, represent a confusing variety in proximal terms. "Instead of presenting a single unvarying and undistinguishable sameness, as is too often assumed, habits . . . present a remarkably varied series of patterns" (**12**, p. 33).

According to Hull, some mechanism has to be found which gives identity to this 'family' of patterns in terms of action sequences. The problem is essentially the same as that which von Kries stated for perception: Is there anything identical in the different stimulus patterns that correspond to an identical object? If the analogous problem were solved for action, it would be a decided advance toward an explanation of goal directed behavior. "It [the principle of habit-family hierarchy] is operative in all situations wherever there is more than one distinct action sequence which will lead to the attainment of a particular goal or subgoal. It is believed, for example, that the habit-family hierarchy constitutes the dominant physical mechanism which mediates such tests of truth and error as organisms employ—that it provides the basis for a purely physical theory of knowledge" (**12**, p. 40 f.).

We do not have to go into the details of this interesting and ingenious theory in order to show why we believe that it, too, achieves the solution by means of a disguised focus. The critical point is the definition of the identical element which makes the different action sequences belong to one family

and thereby interchangeable. This identical element is the 'same' goal reaction which is brought forward in the action sequence as "fractional anticipatory goal reaction." "It thus seems probable that the fractional anticipatory goal reaction is the major mechanism which brings about the integration of the habit-family hierarchy" (**12**, p. 43).

The whole theory depends on the terms in which the goal reaction is defined. If it is possible to define it in real stimulus response terms, that is to say, proximally, and without including environmental determinants, then the theory has solved its problem.

It seems, however, that the goal reaction is not defined in this way. This fact becomes especially clear if we examine the case of 'purposeful' locomotion and orientation in space which Hull discusses in terms of this theory. Which determinants do we have to use in order to be able to say that goal reactions belonging to different action sequences (action sequences which correspond to different paths to the same goal in the environmental space) are all one and the same goal reaction? According to Hull, these goal reactions seem to be distinguished from other goal reactions only in that they all occur at a certain place in the environment, which lies in a specified (again environmentally determined) direction from the starting point. But are these goal reactions identical from the point of view of proximal determination? That would be the case only if, for instance, the movements of the rat in eating the food (*i.e.*, the real response) varied when the rat approached the food from different points of the environment, and were the same when it approached from the same place. In other words we could only make this assumption if there were a differentiation in the movements corresponding to the differentiation of the environmental space, and in some way coördinated to the spatial relation between starting point and goal. Actually, however, the identity of goal reactions is not determined proximally: Two occurrences of goal reaction are called the same goal reaction when they occur at the same place in the environment. Thus, we find that in this theory also the identical element conceals distal determinants

behind apparent proximal determinants; it is a case of disguised focus.

Many critics have objected to the use of environmental (distal) determinants in stimulus-response theories. The following quotations are examples of such objections:

> . . . the conditioned response formula seems to me inadequate in that the two stimuli and the two responses which are picked out in the above example by the conditioned response formula are not, it must be observed, stimuli and responses in any strict physiological sense. They are not physiologically, but environmentally defined affairs. Thus food and string, as visual, olfactory, and tactual stimuli patterns, may be quite different from occasion to occasion. They retain their respective identities from time to time only by virtue of their environmental 'meanings.' And eating and string-pulling, as responses, do not correspond to specific and invariant sets of muscle contractions, but are only identifiable through successive times in terms of environmentally nameable 'manipulations' . . . the conditioned response formula . . . must be loosened so as to allow both 'stimuli' and 'responses' to be identifiable in terms of relatively gross and meaningful characters and not in terms of any precise or necessarily constant sense-organ and muscle processes (Tolman, **20**, p. 200).
>
> These supplementations do more than call attention to the complexity of stimulus-response relationships in conditioning experiments; they introduce terms such as direction, organization, means-end relationships, which are foreign to the logic of stimulus-substitution. To confess that the bald statement of association by contiguity is unsatisfactory because we oversimplify the items which are contiguous is one thing; to supplement the concept with heterogeneous organizing principles not coherent with it is another (Hilgard, **9**, p. 550).

Sometimes it is believed that the variations in the stimuli and responses, which are meaningful if they are determined environmentally, can be explained away by pointing out that there are random variations in every stimulus-response

experiment.[6] However, distal order cannot be derived from proximal disorder. A demonstration of the variations in the stimuli and responses shows only that the function between them is more complex than originally assumed; but it does not explain why it is possible to find distal constants in these proximal variations. Tolman has stated:

> . . . there is a big difference between admitting that stimuli or responses probably vary from time to time and being able to give any account of (in truly stimulus-response terms) why they can nonetheless be called identical with their former selves. It is this latter requirement which I think both Guthrie and Skinner have failed to satisfy (**20**, note on p. 201; *cf.* also **17**, p. 122, and **14**, pp. 344 ff.).

"Why they can, nonetheless, be called identical with their former selves"; that, indeed, is the central problem for any theory using stimulus-response terms. It is the problem of reducing distal terms to proximal terms.

Thus we see that all the theories that employ proximal determinants, whether in terms of perception or of stimulus-response, are faced with the problem of explaining the existence of relevant distal determinants. The weak point of these theories seems to be that they end up by substituting distal for proximal determinants without realizing the change of focus.

II. *Theories in terms of distal determination.*—Distal determination of the entities to be coördinated to the processes in the organism seems at first sight to be entirely possible. Hobhouse, for instance, comes to the conclusion that the organization of observations in distal terms is as legitimate as the organization in terms of proximal determination.

> If a philosopher from another planet, ignorant of all forms of life as they exist upon this world, were to watch a stone rolling downhill and a man running to catch his train, he would come to the conclusion that the stone and the man were actuated by very different principles. He would, for

[6] *Cf.* Hilgard (**9**, p. 373).

> example, see the man go round the obstacle which caught up the stone, and if he proceeded to compare their behavior under many circumstances and in different relations, he would arrive at the result that the broad difference could be most easily formulated by conceiving the stone's action as determined always by the reaction of its inherent qualities upon the forces directly impressed upon it without regard to the ultimate issue, while the man's action would be, in the majority of cases, determined by its relation from moment to moment to some result more or less remote. . . . That is to say, proceeding purely by inferences based on comparison of behavior, he would discover two fundamentally distinct types of correlation, one in which each element of behavior is conditioned by its relation to its result, the other in which no such relation is operative although the result is in fact produced. Now he might ultimately decide that these two types are reducible to one. . . . But even in the latter alternative he would still acknowledge two clearly distinct types of correlated behavior, in one of which the bearing of act on result is operative, while in the other it is not. He would now hold that this relation is made operative by a mechanical arrangement. But operative it still would be, and this would generically distinguish the type of correlation from the type in which there is no such element operating (10, p. 15).

Hobhouse means that, even if we are able to reduce distal to proximal determination, we have to acknowledge that we find in observation cases which give constant coördinations in distal terms. And that is perfectly true, if one limits oneself to a 'majority of cases'; however, there is no distally determined process or movement of the organism which could not be disturbed or made impossible by processes coördinated with proximal events. Pure distal determination is an absurdity; it would mean perception without sense-stimulation and action without muscle-contraction. And worse still, it would be a logical impossibility, because distal determination without coördinated mediating processes could give, in the last analysis, no definite determination at all. If perception is entirely determined by the distant environment, what deter-

mines which object is perceived in a concrete situation? Distal determination seems to imply the impossibility of complete determination and that may be the real cause for the general distrust of distal determinants.[7]

Without doubt, the most imposing system using distal determination is Brunswik's psychology in terms of objects. Its importance lies not only in that it stresses distal determinants, but also in that it offers concepts and methods which allow systematic experimentation. It seems to give the ideal fulfillment to Holt's program, to find "that object, situation, process of which . . . behavior is a constant function" (11, p. 161). At first sight, this psychology seems to combine distal and proximal determination, since the 'real objects' of perception or action are determined both in terms of the environmental objects and in terms of stimuli.

However, we find that the role of the stimuli in mediation and their place 'between' objects and the organism is more or less disregarded. Brunswik writes:

> All of the above facts concerning the functioning of the organism in perception suggest a general way of consideration which would seem to be the one most profitable for psychology. Thus, both for reception and for action, it turns out that the special manner in which anything is mediated (or done) is not especially essential or significant. One and the same means-object may be represented at different times by very different stimulus configurations. And one and the same goal may be reached equally well by very different kinds of movements and means-object manipulations. The focal-points of life occurrences, *i.e.*, means-objects and final goal-effects, lie, respectively: relatively far away in time and space, backward (in cognition), or forward (in action). They are removed from the actual stimulus conditions and the actual body movements, so

[7] The impossibility of complete determination in distal terms is clearly expressed by Brunswik although his own theory is based on distal determinants: ". . . we are dealing with causally distant effects, for which all conditions are not yet ascertained. The relation is therefore not one which is univocally determined in advance but only a more or less probable one. For, it is always possible that unexpected 'marginal' causal chains interfere, or that conditions are absent which one can expect in a normal environment" (6, p. 18 f.).

> that the really significant question always is: What are the kinds of such objects and final goal-effects which the organism is able to attain independently of all the varying circumstances with a relatively large degree of accuracy and probability; achieving them by perception, on the one hand, and by action, on the other. In short, questions of 'what' are much more important in psychology than questions of 'how.' And thus to seek to describe the abilities and performance of an organism by giving an inventory of the kinds of objects attained by it, may be called 'Psychology in Terms of Objects.' In principle, this viewpoint need not have any concern with the organism's actual sensory, nervous, or motor conditions—*i.e.*, with mere mediation problems, as studied in traditional behaviorism, psychophysics, and physiological psychology (4, p. 125).

As we can see, mediation problems, or problems of proximal determinants, have no place in a psychology in terms of objects. At best, mediating entities are considered in their role as good or bad cues for the distant objects (*cf.* especially Tolman and Brunswik, **21**).

What kind of psychology can be built upon such foundations, what determination-tendencies can it satisfy? It is true that it is without fault from the point of view of a positivistic program, and that its statements are verifiable by experiment and carefully grounded in observation. It blocks, however, the tendency to unified determination and it fails to make possible complete determination. Unified determination is not attempted, since its goal is an 'inventory,' a multitude of coördinations which it cannot, and does not, strive to reduce to coördinations of a higher order. Because it only asks 'what' and not 'how,' it cannot achieve complete determination. If it were to ask 'how' and if it wanted to describe completely the processes involved in a single concrete case of behavior, it would have to consider proximal and interior determinants; it is, in the end, even questionable whether it is possible to give a complete answer to the question 'what' without doing so.[8]

[8] Brunswik himself recognizes that 'what' and 'how' problems are intimately connected: "Since the ways of mediation will always determine the achievement, the

These remarks are not intended to belittle the importance of the theory. Its contribution to the development of psychology is a substantial one, since theories emphasizing proximal determinants have neglected to study many distal coördinations, even when, in principle, they have taken account of the fact of relevant distal determinants. There are many fields of psychology in which we are still ignorant of the distal coördinations of behavior, and often the problem has not even been stated properly. The psychology of the mental development of the child, or of language, offers many examples. An experimental method of determining the 'attained objects' of behavior can certainly contribute much to the body of psychology.

III. *A theory in terms of orientation.*—The theory of tropism, as it is presented by Crozier and Hoagland (7), coördinates 'stimuli' with 'orientation,' that is to say, direction which is determined in relation to the environmental space. Not what is closest to the skin-muscle contractions or movements of the limbs—is taken as the focus, but an effect, an achievement of the movements of the limbs. Thus, this theory goes a step beyond pure proximal determination. Determination by orientation lies between determination by the movements of the organs and distal determination in terms of the objects of the environment.

From the following quotations it will be clear that the authors distinguish sharply between proximal determination and determination in terms of orientation, and that they do not think that the second can be reduced to the first.

> Since the anatomical basis for such actions is quite different in diverse organisms, but the behavior element dynamically identical, it is clear that the quantitative formulations arrived at refer to the *behavior*, and not to specific accidents of structure . . . (7, p. 6).

The "anatomical basis" and "accidents of structure" are proximally determined entities; the "dynamically identical behavior element" refers to orientation.

highly abstracted type of object-critical analysis as outlined above would lead, ultimately, to a statement of all psychologically relevant types of 'how'-problems and -findings in terms of 'what,' *i.e.*, of objects attained" (5, p. 251, note).

IV. *Gestalt Theory.*—In order to describe the focal terms of Gestalt theory, we do best to present first the reasoning we find in Chapter III of Koffka's *Principles of Gestalt Psychology.* The question, in what terms one should describe perceptual processes, is put in the form: Why do things look as they do? (**16**, pp. 76 ff.).

1. Distal determinants: "A first answer would be: things look as they look because they are what they are." The method of finding out whether this answer is adequate is to "single out a few aspects of behavioral things and compare them with real ones." That is to say, one may determine how constant the coördinations between perceptual phenomena and distant objects are. Of course, it is easy to find cases in which there are no constant coördinations, such as the moon illusion where there is no constancy. Distal determinants are therefore discarded, because there are cases of disagreement.

2. Proximal determinants: We have to distinguish between (*a*) local proximal determinants and (*b*) non-local proximal determinants.

a. Local proximal determinants. A second possible answer is: "Things look as they do because the proximal stimuli are what they are" (**16**, p. 80 ff.). Again, we find many cases in which there is no correspondence between the local proximal stimulus and the perceptual phenomenon. For example: "The constancy of real things is to a great extent preserved in the constancy of the *phenomenal* things despite variations in their proximal stimuli" (**16**, p. 83).

b. The principle of non-local proximal determination has to be accepted. For example: "If, without a table and even without a light . . . , we could produce the same pattern of excitation . . . which is ordinarily produced on our retinæ when we fixate a table, then the person on whose retinæ these excitations were produced should and would see a table" (**16**, p. 79). That means, in such an experiment we would find the perceptual process coördinated only to proximal events, not to objects.

Thus, both distal and local proximal determinants are discarded; that distal determination is possible in many cases

is recognized but used only in the refutation of local proximal determination. Only non-local proximal determinants are accepted, and that means for Gestalt theory that external determination is made in terms of stimulus patterns, internal determination in terms of fields and Gestalt processes. Koffka states:

> All we intend to do is to replace laws of local correspondence, laws of machine effects, by laws of a much more comprehensive correspondence between the total perceptual field and the total stimulation . . . (**16**, p. 97). Things look as they do because of the field organization to which the proximal stimulus distribution gives rise (**16**, p. 98).

Thus we find the program of Gestalt psychology to be: perceptual processes have to be defined in terms of stimulus pattern and field organizations; the question, "Why do things look as they do?" should be answered in these terms.

Gestalt theory has found a new device for the derivation of distal from proximal determination. As we have seen, several forms of conditioned reflex theory take into account both proximal and distal determinants. They make use of local proximal determination and from it derive distal determination by the device of selection. For Gestalt theory, too, it is possible to take into account both kinds of determinants. However, it does not derive the one from the other in a way which makes the derived focus a spurious one.

Let us consider, as an example, visual fixation and pursuit (**16**, pp. 311 ff.). As long as we determine the coördinations proximally and locally, in terms of peripheral movements and sensations (or local stimuli), we find a bewildering confusion without constancy. Distal determination is in many cases easy: The eye is tuned in on the object world, it follows moving objects, etc. The problem is: How can we anchor distal determination in proximal determination, how can we exclude teleology?

The solution proposed by Gestalt psychology is the following: It is wrong to start with an identification of muscle movement qua movement (local determinants), and then hook

on to these entities connections which are forced upon the organism by the contact with the environment. Muscle movements and stimuli are of course there and play their role in the process, but they can be determined only as parts of an organization. And this organization takes care of the distal determination at the same time. According to Koffka, "... a stimulus inhomogeneity [starts the movement] and the movement takes place in such a way that this stimulus inhomogeneity is brought into the center of the retina" (**16**, p. 313).

The organization in which the local stimulations and the local movements are embedded is of such a kind that it gains its equilibrium when the requirements of distal coördination are fulfilled; that is to say, when the eye is directed towards the object, which is coördinated to the stimulus inhomogeneity. In this way proximal and distal determinants, the local events and their 'achievement,' are brought into harmony. The model of this combination is taken from physics. In physics, too, an event can often be described in both proximal and distal terms.

> We may best visualize the relationship between the responses that make up the so-called purposive behavior category by the rain-drop analogy. We may start with the assumption that every drop of rain in some way or other gets to the ocean. . . . Anthropomorphizing this condition, we may say that it is the *purpose* of every drop of rain to get to the ocean. Of course, this only means that virtually every drop *does* get there eventually. How it gets there depends upon where it falls. . . . Each stage, each fall from one leaf to the next, may be designated as a *means* toward the final end, the sea, and a number of the intermediate stages may be grouped together and the terminal stage designated as the purpose of the antecedent stages. . . . Human behavior is merely a complication of the same factors. Instead of only a few physical forces such as gravity, temperature, humidity, surface tension, friction, that act on the drop of rain, the stimuli which act on the sensori-motor system of man are much more numerous (**22**, pp. 346–347).

Gestalt psychologists can agree essentially with these statements of Weiss. They show that distal determination in physics is in principle identical with that of behavior, and that physics makes use of a device which resolves distal into proximal determination. Thus it does not leave teleology hanging in the air. However, the most important concept, by which physics achieves this end, is not given enough credit in the above quotation. It is the gravitational field and not leaves, ground, or arbitrarily arranged forces which make the movement of the drop of water one which can be determined distally. Field and equilibrium are the concepts by which in physics the distal determination is made congruent with the proximal. The field brings together the end of the movement and the forces which affect the moving body directly and makes them actually one and the same thing. The 'goal' is a unique place within a field at every point of which there are forces directed towards this place. Field determination is really neither distal nor proximal determination; both these determinants are merely aspects of field determination.

The total process of a psychological organization is, of course, much more complicated than the organization of these simple physical examples. Several regions often take part in the total process, regions which have more or less autonomous organizations.

In the actual work of Gestalt psychology, we find a discrepancy between the treatment of perceptual problems and that of behavioral problems. The coördination of the organism to the object world, that is to say, distal determination in terms of the objective environment, is considered and 'explained' in the treatment of the psychology of action. In the treatment of perception, however, the fact of correspondence to the object world is often neglected and the goal or final state, toward which a process is directed, is determined in terms of figural properties.

It is significant that Koffka introduces his discussion of action by a section which is headed: "The results of behavior" (**16**, p. 306). In this chapter we read:

> . . . if we want to explain behaviour, behaviour which has been such a powerful agent in the world, can we ever hope to succeed if, right at the outset, we forget what behaviour has accomplished? That is to say, must we not, in order to explain behaviour, first gain some knowledge about those universal aspects of behaviour which have been responsible for its success? Will it do to introduce explanatory principles indifferent to the results of behaviour, principles which would explain as well, or better, utter chaos . . . ? (16, p. 307).

However, in the chapter on the constancies, we read:

> . . . the connection between this uniqueness of one set of conditions and its cognitive value should not be used in any sense as explanatory of the uniqueness . . . the constancy problem should be re-formulated in this way: What shape, size, brightness, will correspond to a certain local stimulus pattern under various total external and internal conditions? Once we have answered this question we shall know when to expect constancy, when not. Indeed some effects of non-constancy are just as striking as the effects of constancy which have been so much emphasized . . . (16, p. 227).

Does that not mean, that we should "introduce explanatory principles indifferent to the results" of the perception?

We find thus an inconsistency in the attitude towards distal determinants. Especially in perceptual problems, Gestalt theory has limited itself to an investigation of figural organizations, and has more or less disregarded the original program of taking into account the 'achievements' in terms of equilibria. The original program was to make 'meaning' dynamically real and to give a solution for the problem of the coördination of the organism to the object world.

This is also the meaning of the claim of Gestalt psychology to make the 'order' in the psychological processes understandable. Order always refers to some particular kind of determination. Something can be orderly in regard to one determination and disorderly in regard to another. The order, about which Gestalt psychology talks in its general

program, is mainly the order we find when we determine the events distally. As Köhler has said: "All this order is as remarkable as it is necessary for our response to the objects which, in the form of bodily movement, must be adjusted properly to the physical world" (17, p. 115). That other systems of psychology failed to explain adequately the coördination of the organism to the environment, is again and again pointed out by Gestalt psychologists.

However, as we have seen, in the psychology of perception the environmental distal determination is disregarded. The focus is placed in the figural, geometrical properties of the stimulus pattern, and the distal determinants which are used are figural-distal determinants; *i.e.*, the fact is pointed out that we can often describe perceptual processes as tending towards a certain configuration.

We have seen that distal determination does not order the events in the same way as local proximal determination; we have to add it does not order them in the same way as figural determination. That means, when we determine in terms of figural properties we have not yet solved the problem why environmental distal determination is possible. This fact has been pointed out by several authors:

> The same thing can 'express itself' in many different stimulus patterns, which are, also from a figural point of view, different; and we can recognize it through these different mediations (Heider, **8**, p. 384).
>
> One can call this multitude of possibilities a transposibility—perhaps 'sign transposibility.' This is different from the gestalt or sum-transposibility and plays, so far, only an unimportant role in gestalt psychology. For, the different equivalent possibilities do not show common form (figural) properties . . . but only common empirical significance (Bewährung) . . . they have in common their character as a sign of something that is causally more distant than they are (Brunswik, **3**, p. 228).

Thus, we see that Gestalt psychology has developed a new device for the solution of the problem of the discrepancy between proximal and distal determination. However, it has

not made use of this device consistently, and in some cases it has lost sight of the environmental distal determinants. Connected with this is the fact that it has not yet developed a theory of experience in terms of organization; it does not account for the fact that contact with the environment makes the organism more coördinated with it. Of course, the limitation to an intense study of figural processes was probably very wise; one might only raise the question whether the psychology of perception in its present stage would not be advanced greatly by a more extensive consideration of distal environmental coördinations.

D. Summary

In organizing any data one can apply an infinite number of determinants. Scientifically fruitful and relevant determinants are those that yield constant coördinations. This empirical test may lead to rival determinations: At least for a first approximation the same group of data can sometimes be determined relevantly in two or more different ways.

In order to escape the dilemma of multiple determination one kind of determinant is often selected as primary and the attempt is then made to derive the fact that other relevant determinants exist from this primary determination. These derivations follow certain patterns; derivation by selection is a common type. Empirical findings are not always responsible for the selection of a particular determination. We find *a priori* tendencies which favor certain determinants.

Questions of determination are especially important in psychology because there are several regions involved in every psychological process. The rivalry between proximal and distal determinants is one of the most significant for psychological theories. Theories using proximal determinants as the primary ones are faced with the problem of explaining the existence of relevant distal determinants. In the attempts to solve this problem use is often made of the device of derivation by selection. It can be shown, however, that the apparent success of these derivations is brought about by the unnoticed

introduction of distal determinants into the assumptions from which these determinants are supposedly derived.

Theories using distal determinants as the primary ones are faced with the problem of what to do with the fact of relevant proximal determinants. They often state explicitly that this fact has no place in psychological theory. Thus the way to complete and unified determination is cut off.

Gestalt theory offers a framework of concepts which makes it possible to take both distal and proximal determinants into account and which resolves the dilemma of deciding between them. Gestalt theory, however, has not yet made consistent use of these concepts.

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